ADDRESS

TO

THE BRITISH

Board of Longitude.

MATHEMATICAL WORKS OF THE AUTHOR.

I. Introduction à la Philosophie des Mathématiques.

II. Résolution générale des équations de tous les degrés.

III. Réfutation de la Théorie des Fonctions Analytiques de La Grange.

IV. Philosophie de l'Infini.

V. Philosophie de la Technie, première Section, contenant la Loi Suprême des Mathématiques.

VI. Idem. 2e Section, contenant la Préparation à la Réforme des Mathématiques.

VII. Critique de la Théorie des Fonctions Génératrices de La Place.

‡ All the above Works are sold at MM. Treuttel and Wurtz, Soho Square, London.

ADDRESS

OF

M. HOENE WRONSKI,

TO THE

BRITISH

Board of Longitude,

UPON

THE ACTUAL STATE OF THE MATHEMATICS,

THEIR REFORM,

AND UPON THE

NEW CELESTIAL MECHANICS,

Giving the definitive Solution

OF

THE PROBLEM OF LONGITUDE.

RADCLIFFE OBSERVATORY OXFORD.

Translated from the original in French, by W. Gardiner.

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ADVICE.

At the moment wherein this Address is to appear, we receive from the English Government, through the organ of his Excellency Count Lieven, the Ambassador of our Sovereign, an answer to the humble petition we have had the honor of carrying to the throne of His Britannic Majesty, in the sole view of opening to us a way to arrive at the body itself of the Board of Longitude, and to obtain, for the scientific labours that we have brought to England, judges taken even from the bosom of this illustrious Institution. We ought, before all, to express here our gratitude for the interest that this enlightened Government has deigned to take in the justice of a cause, which, perhaps, concerns as much the welfare of science, as the fate of the author.

The principal document in this answer consists of a report made to the Government, by Dr. Young, Secretary of the Board of Longitude. In this report, which is dated the 21st of June last, the Secretary takes upon him, as he declares it himself, to reply, by anticipation, upon that which, according to him, ought to be resolved by the Board in its sitting of November. And in fact, in his letter of the 15th of November, the Board is said to have resolved all that which the Secretary, Dr. Young, had foreseen six months before.

This report made to the Government, and serving for the answer to our petition, is reduced to three principal points as under:—

1. "The Board cannot consent to take "cognizance of a demand which is founded "on the imputation of an *entire* incompe- "tence to fill the functions which have "devolved upon them by an Act of Par- "liament."

2. "The Board has not been able to "find, in the various propositions which "have been made to it by Mr. Wronski, "any foundation for his pretensions."

3. " Nevertheless, if this learned " stranger believes it proper to make " any new communication to the Board, " this body will henceforth proceed with " strict justice in his respect, to decree " him a reward."

Here follows the true interpretation of these three points, constituting the report of the Secretary to the Board of Longitude.

1. Our demand made to the Government, is identically that, which was produced in our *Appeal to Parliament*, published at London in the month of May last, which reduces itself expressly to the obtaining, for the scientific examination of our labours, those of the members of the Board of Longitude, who are occupied, *by profession*, in mathematical sciences.

A single member only (page 18), *and not the entire Board*, is charged in it of scientific incompetence. — Thus, this first point in the report of Dr. Young appears to be contrary to truth.

2. On our arrival in London, the Board of Longitude, contrary to our expressions,

procured for itself the sight of our geodesic instruments, deposited in the Custom House; but we have refused to deliver up the learned theory of these instruments. Thus the Board could fathom nothing of them.

We have, ourselves, only presented to the Board of Longitude one single accomplished scientific proposition, which was that of a new *theory of refractions*. Now, for reasons which will be learned in the present Address, the same was immediately returned to us, without even undergoing an examination by the Board; as is proved by Dr. Young's letter, dated the 27th of April, which accompanied the returned theory. Nevertheless, from this same proposition some advantage resulted to the Board, who has declared to have perceived by it, that the new *Table of Refractions*, which had just been produced in its Nautical Almanac for the year 1822, was false; as is proved by the letter of Dr. Young, dated the 18th of April, at the time of the reception of the above theory, where, on acknowledging the same, he confesses immediately this error in the following terms:—

" I shall give no opinion of my own to
" the Board, except so far as to acknowledge
" that you have detected a *blunder* in my
" hasty Postscript on Refractions.
(Signed) " Th. Young."

We then, at least, can found some pretensions in having rendered, to the British Marine, the signal service of preserving it from the dangerous errors into which it would have been plunged. — Thus, this second point of the report of Dr. Young to the Government, in which he says that the Board has not been able to find any foundation for our pretensions, appears to be, on one part, contrary to truth, for there has been no examination, and on the other, contrary to liberal sentiments, for they have incontestibly profited by our proposition.

3. These reiterated appearances of a constant deviation from truth, are not very encouraging for us to make fresh communications to the Board of Longitude. And we cannot conceive how, by his allegations, the Secretary, Dr. Young, has believed it possible to dispose us to make the like communications, as he seems to desire it in the

third point of his report; unless he is perfectly instructed of the desperate condition into which we are reduced by the British Board of Longitude, and wherein measures of safety the less probable must be tempted. But we ought here to protest, before learned Europe and Posterity, against this new and last compromise, to which we expose great scientific results, in declaring, that, to make this dangerous step, we believe we have sufficient warranties in the invincible force of truth attached to these results, and in the high responsibility that suspends over the Board of Longitude.

Here we may remark, that, in its answer to the Government, the Board of Longitude insists, in two distinct places, on the *exclusive* jurisdiction invested in it by the Act of Parliament. We are far from wishing to decline this enlightened jurisdiction, as our Appeal to the Legislature proves; but we will observe that the predecessors of the illustrious members of this Board, no doubt to render homage to the universality of reason, were not equally jealous of their rights, in the famous judgment of the invention of Harrison. The Duke de Niver-

nois, Ambassador of France in England, was solicited by the British Government to cause to come from Paris men capable of judging this invention, to be joined to the eleven Commissioners of the Board of Longitude; and in consequence of this liberal invitation,—which reflects more honor on Great Britain, than even the prize she decreed,—the Academy of Paris charged MM. Camus, Lalande and Ferdinand Berthoud with this important mission.—Nevertheless, the subject then agitated was only that of a simple piece of mechanism; and not, as in the present case, the principles themselves, or the absolute laws of this part of human knowledge.

NOTE.

The MANUSCRIPT WORK which accompanies this Address, and which is submitted to the Board of Longitude, is entitled : " *First Base of Astronomy, or Laws of the construction of heavenly Globes, presented as the prospect of an absolute theory of Celestial Mechanics*, (a vol. in 4to of 750 pages), to which is joined a *Supplement*, (a vol. in 4to of 180 pages), offering, among other results, *The comparison of the ancient with the new Celestial Mechanics.*

TO

THE BRITISH

Board of Longitude.

MY LORDS AND GENTLEMEN,

Is the reason of man sufficiently powerful, without external assistance, to effect the triumph of truth? Here is, in two words, the question I take the liberty of submitting to your decision.

In the midst of the uncertainty with which humanity has been surrounded, a single ray of real, of absolute knowledge has been imparted to her, to serve as a providential guide in her developement. A thick veil covers, even to the present day, the laws of nature, those of liberty, and especially the laws of the final destination of our species! Man only knows with certainty the laws of space, and those of time or number. Out of this sphere of his mathematical knowledge, he can only grope; nor can obtain any positive physical results, or even morals, but precisely when guided by this light of absolute certainty, which is the character of mathematical truths.

But it is wrong in me to abuse your attention, by proving the infallibility of these truths, and their infinite importance in the developement of human knowledge. You know them both better than myself; and the English Nation, more than any other people, knows how to prize that eternal grandeur reflected upon her by the genius of Newton.

Now!—this mathematical truth, carried to its last perfection, to a perfection as absolute as its own *certainty*, quite undisguised, and indebted to no foreign aid, will it find among the British People, and particularly in their Board of Longitude, that reception it claims? Here, my Lords and Gentlemen, is the same question, but better developed.

I shall refrain from detailing the courses of painful labours and heavy sacrifices, I have necessarily made, to arrive at the term I am now able to signalize. A much greater interest, in the devotion of which I ever forget all that concerns myself, will alone occupy your attention.

During thousands of years, as ancient Egypt testifies, efforts so superior, tending to the developement of the human certainty, this grand attribute of the first of the sciences, remained fruitless. A very small number of mathematical truths were known in that high antiquity; and what unhappily deprived them of their value, is, that this knowledge, as yet, was only *concrete*. Thales appears to have been the first mortal that conceived *abstract* mathematical truths; and the Hecatomb of Pytha-

goras sufficiently proves how great was, even then, their importance among mankind. Nevertheless, it required the revolution of other thousands of years to fill up the interval from the Milesian to Newton. The true science advanced very slowly, both in Greece, and at the school of Alexandria. *Particular* propositions, especially those of Geometry, received alone some developements; though Euclid, Archimedes, and also Apollonius performed prodigies.—Diophantes himself could scarcely soar above the sphere of these simple particular facts.

It is only since the resuscitation of letters that properly begins the *general* consideration, Algebra, and consequently the high algorithmic study of the mathematics.

But, since this first sketch of the powers of knowledge, all the researches were still isolated.—Mathematical truths were, in a manner, *individual;* and Cardan, Bombelli, Fermat, Descartes, and even Kepler, Cavalleri, and Wallis, though these latter had already introduced the idea of the infinite, did no more, as we may say, than accumulate materials for a systematic work that was yet to be produced.

At first, the science was carried from the *concrete* to the *abstract;* afterwards, from the *particular* to the *general.* There yet remained to lift it from the *individual* to the *universal,* to create, in a manner, the true science; and this immense benefaction we owe to Newton and Leibnitz.

From this decisive epocha, the question was no longer about the simple *relation* of quantities, more

or less general, but even of their *generation* itself. And under this aspect, mathematical truths could not be otherwise viewed but in an *universal* manner; for, the generation of quantities, like that of all the objects of our knowledge, ought to be submitted to invariable laws. We thus conceive, that, from this brilliant period, the science arose to consider quantities as true *Beings*, given to reason, and that, by this elevation, she became, at length, in a manner realized materially.

It was then, on his perceiving laws which govern in their generation, the quantities which accompany and rule substances, that man found he could easily become master of the latter. All was submitted to calculation; and the immense progress of the high and true physics, can only be dated from this splendid epocha, where every thing submits to the application of the mathematics.

It was necessary then to discover the modes or laws of the universal generation of quantities. This was the scientific tendency given to genius by the discovery of the method of fluxions.

Already Newton, himself, following this new bent, which had manifested itself since the time of Barrow, discovered, in his grand pursuits, his famous *Binome*. And Leibnitz, equally penetrated by the same impulse, signalized the superior importance of the *Series*. But both failed in finding a true law for this algorithmic universal generation, the high truth of which they so sensibly felt.

In this, Taylor, at length, came to associate his

name with those of Newton and Leibnitz, by the discovery of his fecund theorem, which offers the first universal law of the mathematics, ever known to man; and this discovery properly begins the era of modern mathematics.

Furnished with this powerful instrument, the two Bernoulli, and particularly Euler, worked in regions 'till then inaccessible. This superior man, Euler, it can be said, fixed himself, alone, the whole sphere of the modern science: as far as the application of Taylor's theorem could be extended, so far Euler has, directly or indirectly, unveiled the truth.

It is difficult to divine, what science ought most to have desired, whether Euler had employed his mighty genius in accomplishing, so perfectly, the edifice of another, or directed its creative energies to the foundation of a superstructure still more vast. This, however, is certain, that, beginning to feel the insufficiency of Taylor's theorem, this great geometer sketched out a new mode of universal generation, of which we shall speak hereafter.

This insufficiency was particularly felt in the resolution of infinite equations, or in what is entitled by geometers *retour des suites*.—To supply this defect, La Grange invented his famous theorem, which in this respect completes the system of Taylor, and in others is greatly superior to the theorem of the latter, that is found to be only a particular case of La Grange's.

It was thus insensibly that this grand system of

knowledge developed itself, which, without contradiction, is not only one of the finest properties of humanity, but also one of her most powerful instruments. But what is here so admirable is, that, in making the abstraction of Newton and Leibnitz, founders of this system, we find the last results concentre themselves into *three principal points*:—1st. The theorem of Taylor, as principle of the generation of quantities, when they are given *immediately*, or by their functions:—2nd. The theorem of La Grange, as principle of the generation of quantities, when they are given *mediately*, or by their equations:—and, lastly, or 3rd. The realization of this system by Euler, and the *transition* operated thus from the one to the other of these two theorems.

To this is effectually reduced that immense collection of mathematical knowledge, which is the present property of humanity. Whoever disowns this precise and general reduction, cannot flatter himself, so it appears to us, of having fathomed the science of which we are speaking. In fact, the whole concenters itself thus in this sphere, either in an explicit, or at least an implicit manner; and there exists no proposition, but which we can bring back to one of these three points we have just mentioned.

But the better to appreciate the present state of the mathematics, we must, in ascending thus to principles, discover the only principle which is necessary to serve as base to this system. We may limit ourselves here to the marking this principle;

and in order to that, will observe that the whole of the modern mathematics, since Newton and Leibnitz, consists in the universal generation of quantities, by the sole algorithm of *indefinite summation*, which constitutes the *Series*.

" All that which the method of fluxions gives, this " great instrument in the modern period of the " mathematics, serves in reality but to arrive at this " algorithmic generation by *indefinite summation* : " for, the very small number of theoretic fluents, " which they have been able to obtain, is unde- " serving of notice ; and the only means modern " geometers have in arriving at the knowledge of " quantities, in all their cases, consist manifestly in " the employment, direct or indirect, of this algo- " rithm of the *Series*.*

This precious discovery of that true character of modern mathematics will serve us to unveil, all at once, the cause of the insufficiency of this state of the science, and the prospect of a future one more complete, and, perhaps, even absolute.—But, before we enquire into causes, let us look to effects.

It is notorious that, in defiance of that immense equipage which the modern mathematics display, and which appears to invest them with infinite power for the knowledge of man, the present science is not only insufficient to reply to his high demands, but further, cannot answer the simple wants of his

* Philosophie de la Technie, 2nd section, page 633.

physical existence. In fact, all the great problems remain unresolved; and only to cite here one of them, that of the Celestial Mechanics, one could say, that this problem is there expressly in order to prove this feebleness of modern mathematics.

The cause of this insufficiency is actually easy to be assigned.—We conceive, in fact, that if the science is yet in the simple univeral algorithm of indefinite summation, it cannot reach to all the regions of knowledge. It is here, with the modern science, in her universal generation, as of old it was in the ancient mathematics, when, for the individual generation of quantities, they, as yet, had only addition and substraction. It was then impossible for them to value, with these single rules, irrational and transcendant quantities; as, for example, the relation of the diagonal to the side of the square, and that of the diameter to the circumference of the circle. It is equally impossible, at the present day, to value, with the single indefinite summation, the superior quantities which rule the phenomena of nature.

To well conceive this cause of the insufficiency of the modern mathematics, we must learn that there exists for man many distinct and independent algorithms. The fact is already proved by the impossibility we have just noticed in the valuation of irrational numbers by the means of pure addition and substraction. They were obliged here, in order to arrive at the determination of these irrationals,

to introduce necessarily a new, and essentially distinct algorithm, that of graduation, or of powers and roots. It will then be possible, by analogy, that, in the universal generation of quantities, or in this progress of the modern science, the algorithm of *indefinite summation*, which answers manifestly to simple addition and substraction in the individual generation of quantities, that is to say, to this first step in the ancient mathematics; it will be possible, we say, that this algorithm of indefinite summation, which is still the only instrument of the modern mathematics, may only be one of the divers constituent parts of a system of universal algorithms, which, in their collective strength, might embrace all the possible generations of quantities, and form thus *the absolute system of the science.*

A new reform of the mathematics became then indispensable.—We were obliged to depart from the point of view purely *relative*, in which the science had at first established herself, and whose horizon presented but a limited extent, in order to carry ourselves, at length, to that point of *absolute* prospect, that would enable us to discover all her immensity. This was clearly the last, as well as most difficult stride that remained for the mathematics to make. Every degree of this great science had been already overrun: they had passed from the concrete to the abstract, from the particular to the general, and, at length, from the individual to the universal. One step further remained to be

made, which was that from the *relative* to the *absolute*. But this last was an immense remove, and almost too mighty, considering the actual state of human knowledge.

Nevertheless, the necessity of this grand reform was generally felt, especially so by geometers of the first class, who, occupied in the solution of great problems, acknowledged openly the insufficiency of the modern mathematics, founded only in the algorithm of the series. In fact, in every case they fell upon *divergent* series, from which they could only be extricated by artifices which always injured the generality of problems.—This stubbornness or inflexibility of the series discovered too plainly to them the incapacity of this algorithm for the generation of superior quantities, to which they were desirous of applying them.

Euler was the first, who, feeling sensibly this insufficiency, endeavoured to create a new universal instrument. Thus he brought forth his *Continual Fractions*, which although, as yet, deprived of laws, did not fail, in his able hands, and those of Lambert and La Grange, to change, to say as much, the face of a great portion of modern mathematics. But what we ought here especially to remark, is the tendency in Euler towards this reform of which we are speaking, which tendency he discovers clearly enough when, producing his Continual Fractions, he says:—" Hæc transformatio eo magis est notatu " digna, quod tutissimam ac fortasse unicam nobis

" *viam aperit*, valorem seriei divergentis vero " proxime saltem determinandi."*

After Euler, La Place, a genius not less superior, who had already given the demonstration of La Grange's theorem, and felt, perhaps, still more acutely the necessity of a reform, created a third universal instrument, which he produced under the denomination of *Calcul des fonctions generatrices*. The results which he drew from it are not inferior to those which we have just named. But still what we should here particularly remark, is that tendency, so well pronounced by this geometer, towards a new state of the mathematics, and especially, in some sort, that of the absolute of the science, governed by a single principle; a tendency which this excellent mathematician explains so clearly, when he says: "We cannot doubt but that, in this branch " of the analysis, there is a general and simple way " of considering it."†

At last, nearly all the geometers without distinction, penetrated by the same lively necessity of the reform of the mathematics, the subject in question, employed themselves in the *developement of functions*, superior to those which the theorem of Taylor offered on one part, and that of La Grange on the other, these two last instruments of the science,

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* Nova Acta Acad. Petrop. Tom. II.

† Mémoires de l'Académie des Sciences de Paris, pour l'année 1779.

whose insufficiency they had already so generally felt. Here Burmann, Kramp, Paoli, and especially Arbogast, advanced themselves very far beyond these theorems we have just mentioned. The last of these geometers, Arbogast, introduced even a new method for these developements of functions, under the title of *Calcul des derivations.*

Furthermore, this tendency towards universal algorithms, which might prove superior to Taylor's, concentered at length in the developement of functions, degenerated into abuse. They would reconduct all the science to these simple developements, that should replace the method of fluxions, and extirpate even the very idea of the infinite, that fundamental conception of mathematical science. And this abuse was frankly proved by the first geometer of this epocha, La Grange himself, in his *Calculation of Analytic Functions.*

It was then, when surrendered to these high speculations, we had the happiness to comprehend and well appreciate this uncertain state of the science. We felt that, for to arrive at the term at which they aimed, it was necessary, before all, to fathom the principles themselves of this science, which, to the present day, had served as the model to all others. In fact, the question, in this grand reform of the mathematics, was nothing less than this, to fix an *absolute principle* to those principles of the certainty among men. The task became, in some sort, infinite. We were obliged, this time, in order to

advance the mathematics, and especially to carry them to their final term, which already had been so strongly revealed to man; we were obliged, we repeat, to depart from the mathematics, and seek, out of the circle of this science for excellence, a new support in a superior knowledge, whose absolute certainty should, in its turn, become the base of mathematical certainty.

Happily, about this time, an unexpected reform in philosophy herself began, at length, to be realized among men. Germany astonished Europe by the certainty which, all of a sudden, she knew to attach to sublime truths, which, until this epocha, had remained but simple conjectures with man. The character pronounced by this grand revolution in the human reason, was an open tendency towards the *Absolute itself*. All the sciences, all the superior arts, in a word, all the dominion of man's knowledge was clearly confessed to be but the provisional state, which expected, from this Absolute in question, the peremptory base of its establishment.

As for us, pursuing these grand objects, we had, anew, the happiness to go far enough to arrive at the desired point, where we found, at length, this *peremptory base* of the mathematics, of which we had just felt the necessity.

Established upon a like base, it was now easy for us to discover, all at once, those various principles of the mathematics, after which we had been seeking, and also that final term of the science

where its absolute principle is found, and towards which geometers, during many years, had openly inclined. This discovery was even so easy under the point of absolute view, wherein we had placed ourselves, that we claim no merit from it, and consequently can announce it ourselves without trespassing on the barriers of modesty.

The results of these researches were then, at first, not a new philosophy, for there existed none, but the prime and necessarily the true *Philosophy of the Mathematics ;* afterwards, the complete system of the universal generation of quantities, which we name the *Techny of the Mathematics ;* and, at last, the final term of this science, beyond which there remains nothing further to discover, that is to say, the absolute principle of the science, constituting the *Supreme Law of the Mathematics.*

These three results, upon which we found the reform of the science, are already produced to the public, who alone, without doubt, have here the right to pronounce on them. But it remains with us to facilitate their judgment on this subject entirely new, and whose elements and intimate connexion were first known to ourselves. This obligation we will perform in as few words as possible, as is our duty in this Address we have the honour of presenting to the British Board of Longitude.

The philosophy of the mathematics, whose principal features are traced in the work entitled, "*Introduction to the Philosophy of the Mathematics,*" pre-

sents, *a priori*, a deduction of all the principles of the science, all its various branches, and fundamental laws that govern them. But, as we have not yet given there the positive mathematical results, though they are pointed out, to which this philosophy has conducted us, we cannot submit this work to the decision of mathematicians, who are by no means competent judges of it. It is only to those philosophers, who have followed the last reform in Germany, and who alone can understand us, that it belongs to pronounce upon this philosophy of the mathematics. Also, we have most expressly to request geometers, not to confound, with this philosophy, the positive mathematic results which we present to them, and which we are going to signalize.

The Philosophy of the Techny, the two first sections of which we have published, contains these positive results, which we present to science.—We have already noticed that this techny of the mathematics has for its special object that universal generation of quantities which, since Newton and Leibnitz, characterizes the modern mathematics. But it is the complete and absolute system of this universal algorithmic generation which, at once, we offer. — The algorithm of the series, with which Taylor explored this new career, the theorem of La Grange, the continual fractions of Euler, the fonctions generatrices of La Place, the derivations of Arbogast, &c., are but so many constituent parts of this absolute system, from whence they re-

ceive their true signification and laws, which have remained concealed to the present day. At last, all the other universal algorithms, of which they had not any idea, are found there equally developed, and submitted to invariable laws.

Thus, this vast domain of mathematical knowledge, in which Newton and Leibnitz established the science, and wherein so many geometers have since laboured to extend, is now entirely surrendered to the powers of man. All the various universal generations of quantities are known; and no phenomenon of nature, governed by quantities, can any longer escape the knowledge of man.

But these different universal algorithms ought necessarily to have a common link, one systematic unity, in order to form an homogeneous *ensemble*. And this superior unity, from which flow all these divers and special determinations, can not be any other thing but the *Absolute Principle* of the science. — It is then in this single point wherein ought to be found concentered all this infinite science!—And this is the decisive point we have fixed in the *Supreme Law of the Mathematics*.

This law, being in itself universal, makes a part of the techny of the mathematics. But, as principle, and universal link, it is at once its origin and last term. In fact, in this new branch of our knowledge, all is at first derived from this single law; and, in this same system, all afterwards ends in this single law. Thus it happens that, in this final reunion

of all the various elaborate algorithms, is found *the supreme method of the mathematics*, which we have fixed at the end of the first and second sections of the Philosophy of the Technie.

It is especially by the application of this supreme method that this universal resolution of all the problems, as we have before signalized, becomes possible. In reality, in this method, all the universal generations of quantities are, in their turn, combined and employed distinctly: there cannot, then, be any phenomenon of the world, which, being governed by quantities, but must submit to this supreme method. And it is precisely to give the proof of this important truth, that we offer the present results to the British Board of Longitude.

As for the derivation of all mathematic truths from the single supreme law, we have already proved it by the fact, in having deduced ourselves, rom this infinite law, all those divers truths at present known. And to adduce, instantly, at least a single unexceptionable proof of our different assertions, we declare, that, previous to the publication of our works, having presented to the Institute of France a simple fragment of the supreme law, that learned body could not resist their expressions of surprise caused by this universal derivation of all the science from a single principle. These are their own words :—

" But that which has struck your Commissioners
" in the Memoir of M. Wronski, is this, that he

" draws, from his formula, *all* those we know for " the developement of functions (that is to say all " the modern mathematics), and that all these our " formulæ are but *very particular cases of his law.*"

(Signed) LA GRANGE and LACROIX.*

It would be wrong in us if we were desirous of bringing this erudite acknowledgement, as a decisive proof to the illustrious members of the English Board of Longitude, to whom we have the honor of addressing ourselves. They themselves will know how to appreciate the truth of our assertions, especially as our works are now published, and as this universal derivation of the mathematics from the only supreme law, which has caused the surprise of the French Institute, is found in them, extended not only to all the truths known to the present day, but to all those that, in the domain of the mathematics, are attainable by the abilities of man. We only alledge this foreign testimony to engage the Board of Longitude to suppose as possible, at least, the object of these our divers assertions.

In speaking of the Institute of France, it is quite relevant in us to say a few words on the purport of our polemic works, which occupied us in the interval of the publication of the Philosophy of the Mathematics, and that of the Techny.

We have already strongly remarked on the abuse

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* See the Moniteur of the 15th of November, 1810.

into which geometers, in consequence of the last tendency towards a reform, had fallen, in striving, by means of the developement of functions, to supersede the idea of the infinite. This abuse became so much the more dangerous, as an imposing name was inscribed at the head of these false pretensions. We were obliged then, before the production of our techny of the mathematics, whose true instrument is the conception of the infinite, to restore to this sublime idea its creative faculty of the mathematics. And this was our motive for the *Refutation of the Theory of the Analytic Functions of La Grange.*—We accompanied this refutation with those testimonies of profound respect, which this great geometer had inspired us, but whom we were compelled to attack; and we had the felicity of beholding, at least for this once, that truth triumphed over every consideration.

But this discussion awoke the similar pretensions of Carnot.—Acknowledging indirectly the fallacy of La Grange's theory, this mathematician, more profound than fortunate, believed his metaphysics of the infinite to be sheltered from every attack. Under this delusion, he produced a second edition of his "*Reflections.*" We felt then the necessity of returning to the charge, and, in defiance of geometers, to defend the creative idea which animates their science. This task we performed in the *Philosophy of the Infinite*, in such a manner, that every ulterior

pretension in this respect should be untenable. We had thus the pleasure, even before our Techny of the Mathematics was published, to behold re-invested in her rights, this majestic idea of the infinite, which they had already chaced from all the temples of learning in France, and especially from her superb Polytechnical School.

At length, as we before remarked, among the different geometers who tended towards a reform, he who had conceived, the best, the true object of this reform, consisting in the research of an absolute principle of the science, was the ingenious author of the Calcul des Fonctions Generatrices. And, in fact, as we now see it in our algorithmic Canon (1st Sec. of the Philosophy of the Techny), of which this calcul is but a particular case, the method of these functions was nearer the absolute principle of the science than any other of the universal algorithms which they had discovered: unhappily! La Place, or at least his disciples, appeared to consider this Calcul des Fonctions Generatrices, as being itself the absolute principle of the science. It was necessary, therefore, for to remove all these obstacles, to rectify this too extensive a determination; and this was the motive that produced our *Critique on the Theory des Fonctions Generatrices of La Place.*

In terminating here this explanation of our polemic labours, we ought, in order to prevent the confounding our Refutation of La Grange with our

Critique of La Place, to add the former was to destroy a false doctrine, and the latter only to limit the too wide extension which they would give to one true and full of utility.

As to the *General Resolution of Equations*, which we equally produced between the publication of the Philosophy of the Mathematics, and that of the Techny, it is properly an immediate result, not of this techny of the science, but of the philosophy itself of the mathematics. But, as this general resolution is not yet publicly demonstrated, and besides as it offers a process too complicated in its high generality, we consent, on this point, that geometers might suspend their opinion until they are acquainted with the realization of this general resolution, by its application to the equation of the fifth degree, which is completely finished, and which shortly, we hope, will be in our power to publish.

Thus, in resuming here the only positive results which we have already given to the science, we can determine the three decisive heads, as subjoined:—

First.—The peremptory establishment of the *Techny* of the mathematics, as opposed to the *Theory* of this science, and as offering the *universal* generation of quantities, by opposition to their *individual* generation, which is the object of the simple theory.

Second.—The *Complete System* of all the universal

algorithms, forming this techny of the mathematics; and namely, the respective *demarcations*, and *fundamental laws* of these universal algorithms.

Third.—The supreme law of the mathematics, constituting their *Absolute Principle*, from which is derived in details all the science, and beyond which there exists nothing else in the mathematics.

By the first of these heads, we have introduced a grand division in the science, and attached a precise signification to the modern mathematics, which were founded secretly on this division. By the second, we have accomplished the modern mathematics, which scarcely had begun. And both these, to speak properly, are, as yet, but a developement, and not a reform of the science.

But, by the last or third head, we have departed from all the relative points of prospect of the mathematics, and at length raised them to their point of absolute view, where we have found again all the science in the single law:

$$Fx = A_0 . \Omega_0 + A_1 . \Omega_1 + A_2 . \Omega_2 + A_3 . \Omega_3 + \&c.$$

It is in this supreme law, in this absolute concentration of the science, which enables us to want no more of whatever has hitherto been done, for therein all

is contained; it is there, in this absolute point, which cannot be surmounted, that properly is found the REFORM we introduce into the mathematics. —The reason of man will demand henceforth, that all mathematical truths be deduced from this single law. She will not be satisfied, nor will deem any problem resolved, but when it shall be derived from this only principle, which alone will have the right to impart an absolute certainty, satisfactory to her. And it is this universal and explicit subordination of the science to its absolute principle, which shall form the *new and last period of the mathematics.*

In this respect, the mathematics will precede anew all the other sciences, and thus cease not to serve them as a model; for, all these are, as yet, very far from the absolute state where they shall explicitly have but a single law, or single principle for all their truths.

But let us make here an abstraction of all these high considerations of the reform of the science. Let us enclose ourselves in the Techny of the Mathematics. Further, let us confine ourselves entirely to the supreme law; and, in order to efface even to the least traces of this reform, of which we consent to make an abstracion, let us abandon altogether the name *supreme law*, and even the very name *law*, and give to it only the term *formula.*

Well, this formula, which we have just made known, is completely sufficient for the establishment of a rigorous system of Celestial Mechanics, and

consequently that of a lunar theory, which gives the final solution of the problem of longitude. Thus, offering the accomplishment of the object of the grand institution of the British Board of Longitude, this formula cannot prove indifferent to its illustrious members. For which reason, in this our Address, which we have the honor of presenting to them, we have just traced the history of this formula, leaving it actually to the choice of this enlightened body, whether to consider it as the supreme law of the mathematics, giving the absolute principle of the science, or rather as a simple formula, offering the accomplishment of the institution of the English Board of Longitude.

It shall not be possible, in reality, to contest this last point; and however difficult it is in general to cause truth to triumph, we have the presumption to believe, that, in defiance of all, it is still in our power to establish this—that a rigorous lunar theory is accomplished, and consequently, that the problem of longitude is at last resolved, with an exactitude beyond all that man could desire, and even all he could conceive possible out of this theory. It is superfluous, without doubt, to add here, that we found this our presumption upon the powerful assistance of the British Board of Longitude, to whom we shall reveal, with sufficient warranties, this important result of our mathematical labours.

We will not abuse the attention which we claim from the illustrious members of this Board by re-

tracing here the history of celestial mechanics, to the developement of which we have not yet contributed publicly. We shall limit ourselves to the characterising those results which have been reached in their sphere, and all those at which we may still arrive. But we will do it methodically, by ascending, on one part, to the physical principles upon which, to the present day, is founded this important branch of human knowledge, and descending, on the other, to the mathematical conditions under which, until now, man has been desirous to give order to this modern chaos. We shall be able thus, not only to mark the causes of the insufficiency of the ancient celestial mechanics, an insufficiency confirmed by the very existence of the Board of Longitude; but further, to assign the means of a perfection which we scarcely could have expected.

Among the principles of the celestial mechanics, we must distinguish the phoronomic principles, which serve to fix the simple relation of the motions of the stars, and the dynamic ones, which serve to fix the generation itself of these motions by the action of causes which produce them. — We shall now separately examine the influence of these two kinds of physical principles.

At first, the relation of the motions of the stars may be viewed in divers ways, as is proved by the different systems of the world, imagined from the times of the Egyptians to our own days. The most positive proof in this respect, is presented to us by

Tycho Brahé. Without doubt, we are now far removed from these unreasonable hypotheses; but will it not be possible, that, in the light of reason wherein we walk since Copernicus and Kepler*, there may be different modes of viewing certain motions of the stars, especially in their dynamic deduction? For example, the moon, with relation to the earth, moves about the latter; and with relation to the sun, she moves about that star. Does there not exist any point of absolute view beyond these two points of relative prospects? In this case, besides more pleasing to reason, dynamic laws, which always relate to the absolute point of view, would certainly be found more simple. — But we are so habituated to those considerations purely relative, that we scarcely dare here accuse their imperfection.

Afterwards, the generation of the motions of the stars, by the causes which produce them, finds itself, without doubt, fixed in a manner as true as general, by the admirable Newtonian law. It would be a much greater ignorance than temerity to disown this fine principle of celestial mechanics. For, there does not exist in any science a law, all at once so simple and so fertile as that of universal attraction; and we ourselves cannot give a better idea of what we have signalized before as being the absolute principle of the mathematics, than in shewing, by analogy, this

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* Why cannot we say from the time of Philolaüs and Nicetas?

law of Newton, to be in itself alone sufficient to explain the phenomena of the heavenly motions. But, the more we attach an importance to this fine law, the more we are authorized by reason to demand of ourselves, from whence comes this marvellous fact, or rather rule, so universal in the construction of worlds? for, definitively, this law is not absolute, that is to say, it exists not by itself. In reality, ignorant of the cause of gravitation, we cannot decide, *a priori*, that the attraction in the inverse rate of the squares of the distances is necessary, or absolute: in sticking to the simple physical phenomenon of gravitation, which, by experience, is sufficiently demonstrated, we have not any reason to admit that the law, by which this universal action is exerted, follows rather the relation of the squares, than that of the cubes, or of every other function of distances. But, one will say, it is only by following the relation of the squares, that attraction is able to explain the celestial phenomena, and generally the laws of Kepler. Without doubt; and we do not, by any means, question the truth of this relation; we only remark, that if, for the establishment or the deduction of the law of Newton, we must postulate the laws of Kepler, this precious Newtonian law cannot, as yet, subsist by itself. And as it is repugnant, in some way, to submit to derive it from the laws of Kepler, which, in their turn, cannot subsist by themselves, we ought to strive to ascend again to some dynamic superior principle, which, serving to deduce

both the law of Newton and those of Kepler, without postulating either the one or the other, might carry in itself its proper deduction or principle of its establishment. A like superior dynamic principle, if we could discover it, would clearly be the absolute principle of celestial mechanics; and, in founding ourselves upon this absolute principle, we should naturally be able to introduce, into the dynamic laws which govern the celestial phenomena, a regularity, and especially simplicity, the absence of which is the chief defect of the various theories floated to the present day.

Such is, then, the influence of physical principles in the results that we can obtain for celestial mechanics. Let us now see what is the influence which the mathematical conditions exercise upon these same results.

But let us advise, that, by these mathematical conditions, we do not here understand the methods which geometers can employ for the determination of laws that follow the astronomical phenomena. It is properly the relation in which these phenomena, that are purely physical, find themselves with space and time, which are purely mathematical, that we consider under the title of mathematical conditions of celestial mechanics.

Now, at first, for what concerns space, which finds itself here visibly determined by the dynamic action, constituting, in some sort, the essence of the heavenly phenomena, it cannot, on account of its

foreign determination, be viewed in a manner independent of these phenomena, by which it is engendered. Thus, the modern consideration, introduced by Euler, to reduce the celestial phenomena to three co-ordinates of space, and consequently not to view in it any other thing but a pure geometrical proportion of the variation of these co-ordinates, cannot be, for celestial mechanics, but a relative point of view, which could not be compensated by the generality attached to this consideration. We must then, even here, find an absolute point of view, under which, without entirely neglecting this superior generality, which the consideration of the three co-ordinates presents, we may be able to view the determination of space as proceeding directly from the dynamic action, that is to say, as being, at least in part, the form of this dynamic action.

For that which, afterwards, concerns time, this second mathematical condition of celestial mechanics, it is especially herein we have a criterion, in some sort material, of the greater or less grand perfection of those theories which pretend to explain astronomical phenomena. In fact, in an absolute system of celestial bodies, time, which is the principal part of its form, ought necessarily to be determined by this system itself, and not by any other thing. If it should be necessary to have recourse to another system of heavenly bodies, or any other system whatever of physical phenomena, to be able to determine time in a system given of celestial bo-

dies, this would visibly be the proof that this system, or at least its aspect could not be absolute, since it would depend upon another physical system. Now, this proper determination of time, in a system of celestial bodies, constitutes clearly the *periodicité*, which, in the midst of all the inequalities to which these motions may be submitted, presents, in fact, by their regularity of return, the mean or condition of the determination of time. Thus, according as a proposed theory fulfils, more or less, this superior condition of a system of heavenly bodies, it may, more or less, pretend to perfection.—We ought here to remark, that, among the different geometers who have raised themselves to researches of celestial mechanics, he, who the most sensibly felt this absolute importance of *periodicité*, which we have just pointed out, was the author of the first work entitled *Celestial Mechanics*, who, by that alone, has acquired the legitimate right to attach this title to his work. But however great are those results already which La Place has obtained under this aspect, such as his Equation of Jupiter and Saturn, his Theory of the Satellites, and many others, we must observe, with regret, that this *periodicité* here is only *relative*, and that we want still the knowledge of this *absolute condition*, upon which depends all *periodicité* in general in a system of celestial bodies.

Here, then, in resuming the two physical and two mathematical considerations, are the *four criteria* by which should be judged a theory of celes-

tial mechanics. These criteria ought especially to find a rigorous application, when the theory pretends to fix the absolute state of these superior mechanics. And it is under similar criteria that we offer, to the Board of Longitude, our new Celestial Mechanics, and particularly the Lunar Theory, which we brought to England to reply to the appeal enacted by the Acts of the British Parliament relative to Longitude.

In this fixation of the conditions of the more or less great perfection of celestial mechanics, we have not examined those which depend on a greater or less perfection of algorithmic methods, with which are determined the heavenly motions, their object. These conditions, dependent on algorithmic methods, have already been assigned, indirectly, when before we marked the progress of modern mathematics, and indicated the power of their supreme method. Further, at the end of the first section of the Philosophy of the Techny, under the marks 142, 143, &c. where we have signalized this last method, we have shown, as an example, its application to celestial mechanics, in fixing thereby, definitively, the true signification of that which is called *Epocha*, and especially the signification of *Equations periodiques*, and of *Equations seculaires*.—This application, which, after what we have exposed before, is manifestly the *absolute application* of algorithmy to celestial mechanics, offers thus the true conditions of proper algorithmic methods for these superior

mechanics: and, as such, these conditions form the *fifth criterion* of the perfection of theories which presume to explain celestial phenomena. We may here again submit to this fifth and last criterion the Celestial Mechanics, and particularly the Lunar Theory which we bring to the British Board of Longitude.

It is right, that, after trespassing so long upon the attention of the illustrious members of the Board, we should offer some atonement worthy of their condescension. This we hope to do in the agreeable surprise they will feel on perceiving the extreme simplicity of the results that gives this absolute theory of celestial mechanics, which we announce to them. Here are these new results.

In distinguishing, in every system composed of m celestial bodies, the facts, laws, and data, we shall generally have the following numbers:—

1*st. Number of Facts.*

$$\text{Mechanic facts} = \frac{7}{2} m\,(m-1) + 1$$

$$\text{Geometric facts} = \frac{3}{2} m\,(m-1)$$

$$\text{Total of facts} = 5m\,(m-1) + 1.$$

And taking for unity of the masses that of the central body, and leaving always the unity of space indeterminate, there remains

$$\text{(A) ... Total of facts} = 5\,m\,(m-1).$$

2d. Number of Laws.

Mechanic Laws $= 3\,m\,(m-1)$

Geometric Laws $= \frac{3}{2}(m-1)\,(m-2)$

(B) ... Total of Laws $= \frac{3}{2}(m-1)\,(3\,m-2)$.

3d. Number resulting of Data.

(A) — (B) $= \frac{1}{2}\,(m-1)\,(m+6)$.

Thus, in the Lunar Theory, in considering only the three bodies of the sun, the earth, and the moon, we shall have:

21 = Mechanic facts,
9 = Geometric facts,

30 = Total of facts;

18 = Mechanic Laws,
3 = Geometric Laws,

21 Total of Laws;

$30 - 21 = 9$ Total of Data.

Thus, nine exact observations will be sufficient to determine the special theory of the moon; and 21 Equations, viz. 18 Mechanical and 3 Geometrical, are sufficient to govern the complicated motions of this star.

And such is, besides its mathematical rigour, the simplicity of the Lunar Theory with which we answer the demand of the British Legislature.

It would be vain glory in us to compare with these so simple results, perfectly limited, the complicate, and in some sort infinite results, which, in these superior mechanics, mathematicians, by the ancient algorithmic processes, obtained to the present day.

In introducing the five considerations, which above have offered to us the five criteria to judge of the perfection of a system of celestial mechanics, the results so simplify themselves, that the merit of the production of such an absolute theory is far inferior to the toil which geometers had to encounter in the production of relative theories, where they only could make use of the ancient defective algorithms.*

But, in voluntarily yielding all the merit to others, we ought, for the welfare of science, to insist on the extreme simplicity of our results; which simplicity, besides, in our opinion, wants no apology. Such then is the nature of the new Celestial Mechanics, and

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* As for example, M. Gauss, in the recent production of his Theory of the new planets, in which he is obliged to introduce an infinity of equations, without their embracing completely his subject.—We regret we had not the opportunity of citing before the name of this distinguished geometer, a name attached especially to the Theory of Numbers, which owes to him vast progresses, as we have remarked in our Introduction to the Philosophy of the Mathematics.

especially of the Lunar Theory, which gives the solution of the problem of longitude, and which we offer to the British Board of Longitude.

Well!—but it may be asked where is the proof of this pompous announcement? We reply—in the examination which the Board of Longitude, no doubt, will cause to be made of this theory we offer them. Must there be one or many previous warranties given?—We will produce them.

Let us select one of the most difficult problems of celestial mechanics, and instantly give its solution. But, to join to our pledge immediate utility, we shall take that problem which, at present, occupies most generally the attention of the learned world, and which, by its importance, causes the civilized states of Europe considerable but fruitless expences, as their attempts, which we will shew, are erroneously undertaken, and consequently will ever prove abortive.—It is easy to be guessed, from this reflection, we intend to speak of the *form of the earth*.

We will, then, give here the rigorous solution of the problem of the formation of celestial globes, and especially that of the earth: we will extend it, not only to the determination of the form of these globes, but generally to their construction, both exterior and interior, or in other words, the determination of their figure and structure.—We shall penetrate thus into the bowels of the earth, and demonstrate thereby the whole power of the methods employed in the absolute theory, which is the question.

The results we shall obtain by the solution of this difficult problem, perhaps the most difficult in all celestial mechanics, is competent to warrant an anticipated idea of all the other results of the new theory, by us announced for these superior mechanics, and especially for the lunar motions. We affirm that the results we give here for the construction of celestial globes are *true*, or freed from all hypotheses; *rigorous*, or independent of all approximation; *simple* and *more practicable* than all other results that can be conceived; at last *absolute*, or independent of every foreign consideration, that is to say, definitively, such as the reason of man could desire to know, and consequently as satisfactory in every respect, as ever any other solution of the problem of the formation of celestial globes shall be competent to produce. We affirm, equally, that all the other principal results of our new celestial mechanics, and especially our lunar theory, possess the same absolute character. The learned then will be enabled to judge of the one of these assertions by the other, and thus obtain that satisfactory pledge which, in our reason, seems sufficient.

We repeat, the problem of the construction of the heavenly globes is one of the most intricate, and perhaps most complex of all in the celestial mechanics. We can prove it in two ways.—First, *a posteriori*, or by fact; for, geometers have already obtained results, the more or less satisfactory, in all other problems of celestial mechanics; and they

have, as yet, absolutely nothing in that which is the present question. Without doubt, the fine theory of the earth by La Place, for the superiority of genius with which it is treated, merits our admiration. But nothing peremptory has resulted from it, as is incontestibly proved by those useless processes which are, to the present day, employed for the determination of the figure of our globe. In fact, the measure of the degrees of geographic meridians, to which they have applied in all countries, particularly France, is not precisely the direct means to arrive at this determination. But, what is here decisive, is, that the new measure they intend making in France, of the arc perpendicular to the meredian, from Brest to Strasbourgh, is, without any exception, quite useless in the determination of the figure of the earth, and even that of the limited surface of France. Men of science will be convinced of this truth, by the results we have here obtained, and frankly acknowledge that there exists nothing yet which they have had for the solution of this important problem.—Afterwards, we can prove the superior difficulty of this question, even *a priori*, or by the means required for its solution. In fact, all the other questions of celestial mechanics, bearing upon the motions of these globes, whose construction we are seeking, imply only one independent variable, viz. time, all those motions whatever being finally but functions of time; whilst the construction of celestial globes depends upon two independent

variables, that is to say, two independent ordinates of space, and consequently upon the integration of equations implying partial fluxions. Also, the solution of the problem of Three Bodies, to which are reduced all the problems respecting celestial motions, becomes extremely easy when we introduce into it absolute considerations, the absence of which alone, to the present day, has rendered so difficult this famous problem*.

Whatever may be, more or less, this great difficulty, we think that the manner in which we shall here treat, and resolve the problem of the construction of the heavenly globes, will present a pledge more than sufficient of the new celestial mechanics, which we announce to the Board of Longitude.—But, to accumulate pledge upon pledge, and leave nothing to doubt, we will add another security.

It shall be a second problem of celestial mechanics, equally unresolved to the present day, and such as, by the construction of tables, we may shew the facility with which are reduced, in numerical value, the formulæ given by the new theory.—In truth, the

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* In truth, the problem of the motions of a system of heavenly bodies is incomparably more vast than that of the construction of these bodies; and, for this reason precisely, we cannot, in presenting an anticipated warranty of the new theory of these motions, give the vast theory itself. But this grand problem is incontestibly less difficult, as we will prove by the fact.

extreme simplicity of these formulæ should dispense us from this superfluous pledge. But, to leave nothing to be desired, we will produce this proof, and by it accomplish the whole of our warranties.

This second problem, which spontaneously presents itself after that of the construction of the earth, is the problem of the *construction* of the *atmosphere*, and particularly that of *refractions*, which is so intimately connected with the former.

In fact, the problem of refractions remains unresolved, not only with respect to the means, because astronomers have none, as yet, but approximations, or rather some particular solutions; but further, with respect to the object, because they have still only hypotheses for these imperfect solutions.—We ought, afresh, to admire here La Place's Theory of Refractions, though in the results this theory implicates the double imperfection we have just signalized. In truth, borne by his superior genius, this geometer conceived that the difficulty, or rather impossibility, they had encountered in rendering generally convergent the integral series, which should give the refractions, proceeded from this, that the algorithm of the series was insufficient for the generation of these quantities; and, directed by his bent towards a reform or superior algorithms, he borrowed from Euler a continual fraction*, with which he gave at length, for refractions near the horizon, a

* Novi Commentarii Acad. Petrop. Tom. v. p. 236.

solution superior to all others which had been obtained before him. But this solution is not general, and only applies to the hypothesis which this mathematician adopted for the density of atmospheric strata.—With respect to this hypothesis, we would willingly believe that it is nearer the truth than all others previously produced, but still it is nothing more than an hypothesis; and, only by the solution that we are going to give of this difficult problem of refractions, we can discover the conditions for acknowledging the degree of truth of this hypothesis of La Place.

We think that, by offering anew an absolute solution of this famous problem of refractions, according to the spirit of the new celestial mechanics, without making use of any hypothesis on the construction of the atmosphere, a construction which this solution at length will cause us to discover; and by reducing, even into tables, the very simple formulæ obtained to solve this grand question, we shall give a second warranty of these absolute celestial mechanics.—We affirm still here, as we did with respect to the solution of the construction of the earth, that the results of this new solution of the problem of refractions, such as we give them, are true, rigorous, the most simple, or most practicable that can be obtained, in a word, are *absolute*.

We should be able thus, after having announced and characterized the new Celestial Mechanics, and new Lunar Theory, which we have brought to

England for the solution of the Problem of Longitude, we should be able, we repeat, to terminate here this Address we have the honor of presenting to the British Board of Longitude, if some considerations of the highest importance, which concern as much the welfare of science, as the fate of the author of these labours, did not compel us to entreat these illustrious members to accord us some minutes more of their attention. It is from their resolution, founded principally upon what follows, that will depend, perhaps entirely, the production of these labours.

Until now, the sole object of this Address has been to announce to the Board of Longitude the new Celestial Mechanics, and, *particularly*, the Lunar Theory resulting from them, and to fix two decisive warranties to an announcement so little expected. These will always remain the principal objects of our Address; but, urged by superior reasons, which shall be afterwards explained, we ought to join here an accessary object, which, by the same reasons, we must entreat the illustrious Board to take into their previous consideration.

This accessary and urgent object, in this part of our Address, is to submit, as under, to the English Board of Longitude, the two definitive solutions of the two grand problems, viz. the construction of the earth, and that of the atmosphere, not only as pledges of our new celestial mechanics, but further as simple *proposals useful to science*, in order to

reply to the appeal enacted in the eighth article of the new statute of the Board of Longitude.

We should then, under this new aspect, legitimate the *scientific utility* of the solution that we offer of these two grand astronomical problems. This shall be done as rapid as possible, by following the natural order of considering, at first, the construction of the earth, and afterwards that of the atmosphere, with the refractions that depend upon the latter.

We want not to retrace here the history of the human knowledge on the formation of the earth. The enlightened members of the Board of Longitude, to whom we address ourselves, know these progresses of mind better than we are able to develope them. But we shall here re-ascend into antiquity, for the purpose, at least, of recalling those difficulties that this problem, though in appearance so simple, always presented.

Scarcely can we credit, in the present day, what Diodorus Siculus reports, that, in those hoary times, the Chaldeans believed the earth to be concave; others, to have the form of a timbal; and some even the shape of a reversed pyramid. Anaximander, according to Plutarch and Eusebius, conceived it to be a cylindar of stone; and even Anaxagoras and Epicurus, lovers of paradoxes, durst not, in this respect, venture an idea beyond this, that the earth formed a plain disc.—The prevalent opinion in Greece considered our globe as a cube or an oblong

parallelopiped, from whence originated the idle disputes upon the centre of the surface of the earth, which the Greeks fixed at Delphos, where the two eagles of Jove, which flew from the two extremities of the earth, encountered each other.

The Romans, less inclined to knowledge, admitted, in their turn, these divers opinions. Lucretius sports his wit on the gravity towards the centre, and the existence of the antipodes :

"Sed vanus stolidis hœc omnia finxerit error."

The Jews, according to the prophets, especially Ezekiel, supposed the earth was supported by seven columns; and the primitive christians inherited this error. Lactantius, in the third century, ridiculed the idea of the antipodes. St. Augustine equally censured it.

It was but in the fifth century Cleomedes alledged solid proofs of the rotundity of the earth. But, still in the eighth, Virgil, Bishop of Saltzbourgh, was suspended from his functions, by Pope Zachary, for having supported the opinion our globe was round. — In truth, Pythagoras, Plato, Aristotle, Cicero, and even Ocellus of Lucania had already supposed this true figure of the earth, but with so little reason, that Strabo deemed them unworthy of recital, and Plutarch openly derided their opinion.

These erroneous suppositions did not, however, prevent attempts, even in the earliest antiquity, to ascertain the measure of a degree of the meridian, in order to induce from it the size of the earth, ac-

cording to the less unreasonable opinions they had of it.

The most ancient measure, falsely attributed to Anaximander of Miletus, is that related by Aristotle. The second, better known, is that of Eratosthenes, from Siena to Alexandria, in an extent of 7° 12′. The third evaluation, purely hypothetical, and whose results are too small, was made by Possidonius, friend of Cicero, between Rhodes and Alexandria, by means of the altitude of Canopus. At last, the fourth measure, before the extension of Vandalism, was ordered by the Kalif Almamum, at the beginning of the eighth century, on the plain of Singar.

Very late after the re-birth of letters, in 1550, J. Fernel, a French physician, measured a degree of the meridian; and thus opened again this fine career of science.—We must say, from this period already, France roused herself to develope this department of the human mind: it would be unjust to refuse her a principal share in the progress of our knowledge on the construction and form of the earth.—After Fernel, Snell in Holland, Storwood in England, Riccioli in Italy, and at length Picard again in France, the more and more obtained better determinations of the extension of a degree of the meridian.

It was then that the consideration purely *geometric* of the form of the earth began to give place to its *mechanic* consideration. The influence of the rotation of our globe upon its form was, at this time, almost generally felt. At first, the question was only

of the diminution of the gravity from the poles to the equator. It was to verify this important phenomenon, that Richer, sent to Cayenne by the French Academy, observed, in 1671, the variation of the length of the pendulum of seconds. Halley, in 1675, made similar observations in the island of St. Helena. But very soon, Newton's opinion upon the compression of the earth, concluded from this new mechanic consideration of the form of our globe, was generally extended.—Much more, this fine discovery of Newton was undeniably verified by that of Cassini, made in 1691, consisting in the agreement of the rotation and compression of the planet Jupiter.

Nevertheless, this illustrious Cassini, founding his opinion on the measure of the meridian of Paris, extended in 1683 to Colioure, and in 1718 to Dunkirk, believed he ought to oppose the opinion of Newton, and give to the earth a lengthened form in the sense of its axis. This grave error, legitimated in some sort by the inaccuracy of the geodesic measures, however resulted to the great advantage of science. In fact, to clear up this important question, the Academy of Paris proposed to measure the curvature of the meridian, under the equator, and at the pole. In consequence of this proposal, two learned parties of Frenchmen,—the one consisting of La Condamine, Godin, and Bouguer, to whom was joined the celebrated Antonio de Ulloa; and the other of Maupertuis, Clairaut, Camus, Le Monnier, —departed, the first to Quito, within 13′ of the

equator, the other to Torneo, 24 degrees from the pole.—The results of their measures at length fixed positively the opinion in the learned world of the compression of the earth.

The question was then no longer, during the remainder of the last century, but to confirm every where these measures of the degree or of the curvature of the meridian. Thus, La Caille at the Cape of Good Hope, Mason and Dixon in Pensylvania, Boscovich and le Maire between Rome and Rimini, Beccaria in Piedmont, Liesganing in Austria, and other astronomers and geometers reproduced these measures of the curvature of meridians. But those determinations which incontestibly merit the most of our attention, are the four following:—

1st. In France, the grand measure of the meridian, by Delambre and Mechain, which gives, for the degree in the middle latitude of 45°, the length of 60779 fathoms.

2nd. In Sweden, by Melanderhielm, which gives, for the degree of the meridian in latitude 66° 20′, the length of 60955 fathoms.

3rd. In England, by General Mudge, which gives, for the degree of the meridian, in the latitude of 52° 2′, the length of 60820 fathoms.

4th. In the East Indies, the grand measure of the meridian, by Colonel Lambton, which gives, for the degree in the middle latitude of 11° 37′, between Namthabad and Punnœ, the length of 60481 fathoms.

But what was the surprise of astronomers when, upon comparing these divers measures, they perceived their general discordance. — They were obliged then to acknowledge that the form of the earth is not that of a regular ellipsoid.

Further, this discordance of the curvature of different meridians is found such, that we even cannot suppose that the form of the earth should be that of a rotarary spheroid. — It follows that the terrestrial or geographical meridians, answering respectively to the celestial or astronomical meridians, are curves of a double curvature; and that such are also the equator and parallel circles.

Unhappily, reiterated efforts could not advance the theory itself of the construction of the earth, in a manner equal to the increasing complication of those various geodesic determinations that we have just signalized. In these continual theoretic essays, geometers arrived successively at new difficulties, which they could not remove but by new hypotheses.

Thus, Newton already, in his *Principia*, introducing the hypothesis of an homogeneous mass, attributed to the earth an elliptic form, and valued its compression at $\frac{1}{230}$. Mac Laurin, in his Memoir on Tides, gave a deduction, in all forms, of this erroneous supposition of Newton; and the same was followed by d'Alembert, Simpson, Clairaut, Bouguer, Boscovich, and others. Nevertheless, d'Alembert, already in 1768, in his *Opuscula*,

remarked, that, for a like rotation, a spheroid might receive two compressions, the one of which, very considerable, would, in an homogeneous spheroid, reduce the axis to $\frac{1}{681}$ of the diameter of the equator.

However, towards the year 1772, geometers began to doubt of the elliptical figure of the earth, and endeavoured to trace out whether the form of our globe might not be different from that of the ellipsoid. It was then that Le Gendre and La Place, in their Memoirs to the Academy of Paris, for the years 1782 and 1784, demonstrated that, in the hypothesis of an homogeneous mass, the ellipsoid was the only form that could agree with the figure of the earth. Le Gendre, in 1790, even extended this proposition to the general case where the earth would be composed of concentric strata, having different densities.

But, on one part, the diminution of the weight from the equator to the poles, such as is marked by the length of the pendulum, was not found to accord with the theory of the homogeneousness of masses. Already, Newton himself remarked (vol. iii. page 112) this defect of the theory, by advancing the fine proposition, that the total augmentation of the weight, from the equator to the poles, was ever equivalent to the compression. Now, this augmentation of the weight, estimated by incorrect measures of the length of the pendulum, appeared to give a compression greater

than that of $\frac{1}{230}$, which resulted from the hypothesis of an homogeneous spheroid. La Lande valued it thus from $\frac{1}{181}$ to $\frac{1}{185}$.

On the other part, the geodesic measures more recent*, gave a compression smaller than $\frac{1}{230}$, which, according to the received hypothesis, was generally adopted. In fact, at this time, La Place found that the various measures of meridional arcs agreed the best with a compression less than $\frac{1}{250}$; and Boscovich pretended even, that these divers geodesic measures could not conform but to the compression of $\frac{1}{341}$.

The perplexity of geometers became then very great. Nevertheless, the same Boscovich, in his *Astronomical Voyage*, thought it advisable to introduce the hypothesis of a spheric kernel, forming the central mass of the earth, and having concentric strata of increasing density; and thus conceived he could value the compression of this globe at $\frac{1}{335}$; a result which brought him very near to that which he had obtained by the combination of geodesic measures.

It was then that Clairaut produced his *Theory*

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* We say more recent, for, the measures made at first, under the equator and polar circle, gave, according to Bouguer and Maupertuis, the former a compression of $\frac{1}{179}$, and the latter that of $\frac{1}{179}$.

of the Figure of the Earth, in which the principal result (pages 243, &c.) consists in this theorem:—
" If we conceive the earth composed of an infinity
" of elliptical strata, having whatever densities, and
" compressions, and if we consider, as the term
" of comparison, the $\frac{5}{4}$ of the centrifugal force,
" that is to say $\frac{1}{230}$, the sum formed of the com-
" pression of the ellipsoid and total augmentation
" of the weight from the equator to the poles,
" ought always to be equal to this term of com-
" parison."

According to these ideas, La Place, combining the various lengths of the pendulum, and fixing thus its increase, the most probable, arrived, already in the year 1783, to deduce from it a compression of the earth equal to $\frac{1}{321}$. And such were, very near, the results of these reiterated theoretic efforts, until the time when, in his Celestial Mechanics, this same geometer, collecting these divers ideas, and rendering them systematic and general, produced his fine theory of the construction of the earth, which we have cited before.

Unhappily, according to what we have already remarked, nothing peremptory has resulted from this theory for the determination of the form of the earth. The two principal results to which La Place fixed thus the actual state of our knowledge on the construction of the earth, are the following:

First, at No. 30 of this theory, after having re-

duced his general considerations to that of a spheroid entirely fluid, such as ought to be originally the heavenly bodies, having an increasing density and a decreasing ellipticity from the surface to the centre, La Place fixes the limits of the compression of this spheroid between $\frac{5}{4}$ and $\frac{1}{2}$ of the relation of the centrifugal force to the weight.

Second, at No. 34 of the same theory, after having developed, in this species of spheroids, composed of concentric strata, the law that the weight follows at the surface of these bodies, and after having expressed this law in the function of the length of the pendulum, this geometer deduces from it the famous theorem of Clairaut we have before related.

This is the whole which results from this fine theory of La Place, and this also is the whole to which is actually reduced the human knowledge upon the construction of heavenly globes. In fact, on one part, placed between the two limits of No. 30, La Place conjectures, at No. 34, that the compression of the earth is less than the $\frac{5}{4}$ of the centrifugal force, that is to say, less than $\frac{1}{230}$; and only by this conjecture, he explains the result of geodesic measures, which he values, in No. 41, at $\frac{1}{334}$ by the combination of the measures made in Peru and France, and at $\frac{1}{312}$ by the combination of all the principal geodesic measures. And, on the other part, founded on the theorem of No. 34, he can

conciliate with this compression of the earth, given by geodesic measures, that which results from the increase of the pendulum from the equator to the poles, and which he values thus, in No. 42, with more appearance of truth, at $\frac{1}{335}$. To this actually is limited all the knowledge he gives us on the construction of the earth.

Now, the first of these results of the Theory of La Place is evidently but an hypothesis, founded on the gratuitous supposition of an increase of the density of the earth in regular concentric strata. And the second of these grand results, although purely algorithmic, is an error.—We may conceive, in fact, that there exists no reason for admitting a like regular distribution in the interior masses of our globe; at least, this gratuitous supposition deprives us of the advantage of a general consideration in the structure of celestial bodies. As to the theorem at No. 34 of the Theory of La Place, forming the second grand result, we prove, with mathematical rigour, in the work subjoined, which we submit to the Board of Longitude, that this theorem is erroneous,* that is to say, that a similar relation be-

* The error of La Place, Clairaut, and all other geometers who have professed this theory, proceeds from the following causes. Not only in an homogeneous mass, but generally in any heterogeneous one whatever, it happens always that, in the formation of a celestial body, two distinct laws can establish themselves for the weight at the surface of this

tween the centrifugal force, the compression, and augmentation of the weight, from the equator to the poles, cannot take place in an ellipsoid, whatever may be the distribution of its interior masses, concentric or eccentric, regular or irregular. We find again, on the contrary, that the aforesaid theorem of Newton is true for all ellipsoids, whether homogeneous, or heterogeneous in their masses.

Thus, far from having advanced our knowledge on the true construction of heavenly globes, these

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body. According to the one, the weight increases, and according to the other, it diminishes, from the equator to the poles; but both of these changes are respectively operated in proportion to the square of the sine of the latitude. In the first case, the mass forms itself into a spheroid; and in the second, into a ring, similar to those of Saturn. Now, the difference in this consideration reduces itself to that of the sign of a square root. Thus, failing this sign, or what depends upon it, they confine themselves to the consideration of the ring, instead of following that of the spheroid; and, confounding then both these considerations, they fall into error.

We ought here to say, that the theory of La Place on the rings of Saturn, which he assimilates to nuptial rings, is by no means exact.—It results from the absolute theory, that, in their formation, heavenly rings are governed by the same laws as spheriods, namely, that these rings are but spheroids emptied in the sense of their axis. The compression is not in them an indispensable condition; it may happen of rings making a part of a sphere, or even of a spheroid infinitely lengthened; and then they present the phenomenon of terrestrial *water-spouts*.

repeated theoretic efforts have only reached to hypotheses, or even errors. The principal defect of these theories is precisely that of their want of the true law which governs the weight at the surface of celestial bodies, considered generally as having whatever form, and consequently, whatever distribution in their interior masses. Furnished with this law, geometers would have been able, with as much ease as truth, to discover the compression of an ellipsoid by the single datum of the weight at its surface. They would thus have reached to distinguish, in our globe, a compression which is proved by the absolute length of the pendulum, and another equally so by the curvature of meridians on the continents; a distinction which would not have failed of casting a grand light upon these irregularities they have met with in the figure of the earth, and which result principally from this complication of the compressions of our globe. In fact, it is necessary to know that the two fundamental data which we have just mentioned, that is to say, the curvature of meridians, and the weight at the surface of the earth, cause us to discover, by the help of the new theory, two very distinct compressions in the form of our globe. We will limit ourselves here to the production of these different compressions that are given, for the supposed ellipsoid of the earth, on one part, by the four measures we have just cited, and on the other, by the observed length of the pendulum vibrating seconds. They are as follows:—

I. *Compression at the poles, resulting from the geodesic measures.**

Combined with the East Indian measure.

1st. From the Swedish	$\frac{1}{307,19}$	The mean $\frac{1}{310}$.
2d. From the English	$\frac{1}{313,54}$	
3rd. From the French	$\frac{1}{309,15}$	

II. *Compression at the poles, resulting from the length of the pendulum.*†

From the observations made:

1st. In Quito	$\frac{1}{577,02}$	The mean $\frac{1}{578}$.
2d. In Paris	$\frac{1}{578,10}$	
3rd. In London	$\frac{1}{578,87}$	
4th. In Kola	$\frac{1}{579,54}$	

Thus, the curvature of meridians on the continents proves incontestibly, in our globe, a compression of $\frac{1}{310}$; and the weight at the surface of this globe, measured by the length of the pendulum, proves, in the same undoubted manner, a compression of $\frac{1}{578}$‡.

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* According to the calculations of Colonel Lambton, in the Philosophical Transactions for the year 1818.

† According to our new theory.

‡ It is remarkable that this last compression is the same which Huyghens had already supposed.

It is necessary to know, in fact, that this last result is obtained by an absolute theory, independent of all conjecture, or of all hypothesis whatever, and also materially, in some sort, as the first result is obtained by the curvature of meridians.

That which puts to the utmost height this complication of the form of the earth, or at least its manifestation, is, that the increase of the pendulum, from the equator to the poles, such as it takes place on the continents, regulates itself in part according to the absolute weight at the surface of the earth, and in part according to the form of the continents. From whence it results that this increase of the pendulum shows necessarily an inaccurate compression in the hypothesis of a regular ellipsoid, composed of masses whatever. Also, the new theory discovers, that this increase of the pendulum, or of the weight on the continents, from the equator to the poles, gives an inaccurate or discordant compression of about $\frac{1}{175}$; and not, as results from the erroneous theorem of La Place, a compression of $\frac{1}{321}$ or $\frac{1}{335}$.

We may easily conceive that these divers complications in the form of the earth, and especially in the manifestation of this form, are precisely the causes that have rendered so difficult the theoretic explanation of the construction of our globe. By inclining constantly towards a single ellipsoid, or towards an unique compression, geometers have only sought to reconcile the divers contradictions

in the results they have obtained for the form of the earth. And on the contrary, it is in the precise distinction of these pretended contradictions, that we shall, at least in part, find the explanation of the phænomenon which the construction of the earth presents.

We may conceive, by the same, that the processes which geometers have employed for the determination of the form of our globe, ought to be, if not inexact, at least insufficient and useless.—Thus, not only the divers measures of limited terrestrial arcs, but even the ulterior extension of the measure of the meridian of France, by Messrs. Biot and Arago, as well as the new measure of the perpendicular arc from Brest to Strasbourgh, if it be to take place, are nothing but provisionary measures, which will remain perhaps quite useless in the determination of the form of the earth.

Such is then the actual state of this problem, or that of our knowledge in respect to the formation of our globe. — The result from it is, that not only we know not yet the figure of the earth, but further we grope by chance, in accumulating measures of meridians, and perpendicular arcs, which have not any relation to the determination of the figure of our globe. In fact, the form of the earth not being that of a rotarary spheroid, and presenting here great deviations, there is no reason that the curvature of meridians, and that of perpendicular arcs, rather than the curvature of any other arcs, should constitute true data for the determination in

question.—Furthermore, we cannot conceive any idea of the ellipsoid that might be compared to the earth, and consequently none of what, in our globe, ought to be considered as the compression of the earth, or as the radius of its equator. Thus, for example, when following the custom of astronomers and geometers, we take the mean term of the three aforesaid compressions, concluded from geodesic measures, and when we consider this mean, viz. $\frac{1}{310}$, as being the compression of the ellipsoid conformable to the earth, this is only a voluntary fiction, that contains no reasonable sense, or in other words, for which there exists no corresponding reality.

We must conclude, therefore, that, to the present day, the problem of the form of the earth remains unresolved, and that, as yet, we cannnt be assured of any thing of this form.

Now, it is the rigorous and absolute solution of this problem that we offer subjoined, AS THE FIRST BASE OF ASTRONOMY, to the British Board of Longitude.—In a few words, here are the results which we have obtained by it.

At first, with respect to that which concerns the data, it results from this solution, that but a single geometric datum and one mechanic, alone, are sufficient for determining the construction of the earth. But the geometric datum is not the curvature of the geographic meridians, what has been falsely believed to the present day. We must distinguish, besides the astronomic meridian, fixed in the celestial vault, and the geographic one, which

corresponds with it on the surface of the earth, by the direction of verticals; we must distinguish, we say, a *geogenic meridian*, whose curvature is properly the true geometric datum for the determination of the form of our globe.—As to the mechanic datum, it consists in the intensity of the gravitation, determined by the length of the pendulum.

Afterwards, with respect to that which concerns facts, our solution conducts to five superior results, which are as follows:

The first result is, that the form of the earth, which principally depends on the distribution of masses in the interior of the globe, may receive very great variations from very small changes in the distribution of these masses. It follows that the earth's figure might be very irregular: it could even happen, that, in every sense, these irregularities might become, in some way, comparable to the compression of the earth.

The second result is, that if, in defiance of this facility in the production of great irregularities, it should be found, by chance, that the form of the earth might differ but a little from the regular ellipsoid, it would give birth to laws quite special and yet perfectly unknown, that we discover in our solution, and according to which alone this ellipsoid would establish itself conformably to the figure of the earth.

In all the cases, whether the irregularities of the form of the earth be great or small, there will

exist always, on its surface, a train of privileged places, passing from one pole to the other, and crossing the equator, such that their geographic and geogenic meridians shall there be confounded together.—The points of the equator, where this curve passes, present analogous points to those of the equinoxes in the celestial vault, and undergo probably, like these latter, a precession for a long period, on which necessarily depends the magnetic, and generally the meteorologic phenomena of the earth. This, however, is already certain, that we discover, in these privileged points of the terrestrial equator, an origin for the measure of geographic longitudes, or the establishment of a true *first meridian*, which, for such a length of time, has been sought for in vain.

The third result of the new theory is, that the continual change to which the form of the earth is subject from the influence of external causes, namely, the periodical variation of the surface of the waters, constituting the phenomenon of the flux and reflux of the sea, and resulting from the action caused by the sun and moon upon the surface of the globe, is not a simple anomaly, or, as is vulgarly called, a perturbation of the form of our earth, but an integral part of this form, governed by the same laws which preside in the general construction of the earth and all other heavenly globes.—We ought to banish, from celestial mechanics, the barbarous term *perturbation*, with which geometers

have supplied their want of absolute knowledge of the system of the world. There is not, in the whole sphere of celestial mechanics, any thing that ought to be suffered under the title perturbation. All is there submitted to systematic laws, which, in each re-union of celestial bodies, found a perfect *ensemble*, an absolute system, such as it ought to be in reality.

The fourth result is, that if we compare, on one part, the determinations *a posteriori*, that are obtained for the form of the earth by the geodesic measures, which give $\frac{1}{310}$ for the compression of our globe, and on the other part, the determinations *a priori*, that are obtained in our new theory by means of the length of the pendulum, which gives $\frac{1}{578}$ for this compression; we are compelled, before the application will be made of this absolute theory to the definitive determination of the true form of the earth, whatever be its nature; we are forced, we say, to suppose *two distinct states* in this form: the one hydrographic, or the state of the waters, that is to say, the form of our globe shown by the surface of the seas, to which belongs the compression of $\frac{1}{578}$; and the other geographic, or the state of lands, that is to say, the form of our globe shown by the surface of the continents, to which belongs the compression $\frac{1}{310}$. Consequently, from this necessary supposition, we are led to distinguish, in the form of the continents, the ancient state of our globe, such as took place

even at the formation of the lands; and, in the form of seas, the actual state of our globe, such as takes place in consequence of its present rotation, that must materially differ from the ancient one, from which precisely results that diversity in the conformation of the continents. Thus, as this change of the rotation of the earth could not have happened but from some external cause, it is natural to suppose the alteration to have been owing to some shock, or at least to an equivalent action by another celestial body, namely, a comet; which supposition would confirm positively the traditions relative to the great catastrophe or universal deluge, and besides completely explain the geologic phenomena, respecting an antediluvian world, which otherwise cannot be well conceived. It is then probable that the axis of the earth, must equally have received some displacement, and that the revolution itself of our globe around the sun, must likewise have suffered some alteration. We may conceive, that when, by the application of the new theory, there shall be obtained the exact determination of the form of our globe, we shall find again the ancient position of its axis, which perhaps has some analogy with the actual magnetic axis of the earth. —As to the alteration of the annual revolution of our globe, or of the duration of the ancient year, we shall signalize its indices in our celestial mechanics. — Nevertheless, we can, even at present, assign the ancient time of the rotation of the earth,

to which must be attributed the special form of the continents, that is to say, we can already fix the length of the day of the antediluvian world. It was about 17 hours, and 32 minutes, of our actual measure of time.

But, we must not lose sight that these great results cannot be verified definitely, but by the effective application of the present absolute theory to the general determination of the form or construction of our earth.

At last, the fifth result of this theory of the construction of celestial globes is, that by this known form of the earth, regular or irregular, simple or complex, we can discover immediately the distribution itself of the masses in the interior of our globe, that is to say, the interior structure of the earth.—Thus, we shall, with an astonishing facility, penetrate into these mysterious retreats, where, distant from light, the plaistic mother, in her chambers of silence and obscurity, prepares in great measure the generation of all that animates this our globe; and into which the most unbridled imagination has not dared to enter, but with fear and trembling to shadow her fanciful chimeras.

Whence it follows, that we consider generally the problem in question as being that of the *construction* or even *formation* of the earth. In fact, this problem, to become absolute, must necessarily embrace the interior and exterior structures of the globe, and in such a manner that the

latter may be only a corollary of the former. Also, it is found, that if we distinguish the primitive gravitation, or that which would take place if the earth had no rotation, and the derived or remaining gravitation, that results from the influence of this rotation, the knowledge of these two single data, which are purely mechanic, is sufficient for the perfect determination of the structure of the earth, whether interior or exterior. The problem thus is proved to be entirely *mechanic*, and in no way geometric, as has been hitherto believed.

Unhappily, this datum of the primitive gravitation cannot, in all places, be obtained with facility. It depends on the free fall of projectiles at the surface of the earth, in having respect to the rotation of the globe, from whence results that deviation towards the east, of which Newton had already conceived the idea in 1679, and which the experiments of Guglielmini and Henrenberg have recently proved. We give the theory of this motion of projectiles, according to that of a star independent of the earth, with all the perturbations resulting from the resistance of the air, and thus discover in it that other deviation towards the south, experience has made known, but which La Place, led into error by his own theory, pretended could not exist.

But, as we have just remarked, localities do not, every where, admit the obtaining easily this datum,

which is constituted by the primitive gravitation. We must, therefore, replace it with a geometric one, that is intimately connected with it; the obtaining of which is every where easy, as that of the second mechanic datum, that is to say of the derived or remaining gravitation, which immediately results from the length of the pendulum.— Now, this geometric datum is precisely the curvature of the geogenic meridian, which we have before mentioned.

As to the valuation of the curvature of this geogenic meridian, which proves always a certain arc of a great circle at the surface of the earth, it could generally be obtained by the ordinary geodesic processes, in determining the geographic longitude and latitude of the extremities of this arc. But these operations being obliged to be applied at a great number of points of the earth's surface, and requiring a great exactitude in their execution, would become difficult to realise in the time wanted. — Fortunately, we have discovered, that, by the convenient application of the micrometer, we may, without the necessity of knowing the geographic latitude and longitude, determine easily the curve of every terrestrial arc, and that with an indefinite exactness, as great as could be desired, and incomparably superior to what the combined geodesic and astronomic processes can give. Consequently, having developed this new theory of the micrometer, wherein we enter into

all the variations, produced on this instrument, that remained, in a manner, unknown to the present day; and having caused to be constructed in France, at a great expence, a system of micrometric instruments, such as are necessary for the easy obtaining the measure of the arcs of the earth, we brought to England, besides the Celestial Mechanics, our new theory of the micrometer, and this system of instruments, the two latter of which we propose submitting to the Board of Longitude, as soon as its illustrious members shall deign to approve this rigorous solution of the construction of the earth, which at present we have the honor to offer them; or in other words, when they will thus have acknowledged the *indispensable necessity* of this micrometric system, for arriving at the prompt determination of the form and interior structure of the earth.

We should suppose that, by extending these proceedings in her immense colonies, Great Britain might, almost herself alone, collect, in the space of ten years, sufficient data to acquire, at length, that precious knowledge, the true construction of our globe. In the mean time, these proceedings become indispensable for the exact determination of the surfaces of the different political states of the earth, as we show, in the subjoined work, by an example taken from the application of these new processes for the determining the surface of France.

We will now survey the scientific utility of the second grand astronomic problem, that of the construction of the atmosphere, and determination of refractions, the rigorous solution of which we offer equally to the English Board of Longitude.

The history of this phenomenon is neither so rich nor interesting as that one of the construction of the earth.—We have nothing here to recal from antiquity that merits attention, except it be what we learn from Seneca of the division of the atmosphere into three regions, but which we must not confound with a like division of the moderns, though quite analogous. We can, therefore, proceed immediately to a rapid exposure of the different theories, which alone, under our present point of view, merit the attention of the members of the Board of Longitude.

The first discovery of the true infinitesimal element of refractions is due to Euler. This skilful geometer made it known in his fine memoir, inserted among those of the Academy of Berlin, for the year 1754, and entitled: "*On the Refraction of Light in passing through the Atmosphere.*" But, at that period, he had it not in his power to provide for it a satisfactory integration, and was, therefore, reduced to use a simple assymptotic approximation, which, however, did not fail reflecting great lustre on his genius.—The error of this theory of Euler was many minutes for ho-

rizontal refractions, and even in those at some distance from the horizon.

Bradley was the first astronomer who obtained useful results. His process, given in the Philosophical Transactions, is not altogether empirical: it is founded on the principle of the constancy of the angle at the centre of the earth, which he values at $\frac{1}{7}$; a principle equally supposed by Simpson and Boscovich.—The constant co-efficient in the rule of Bradley, is one of the ingenious perceptions which reflects so much honor on this astronomer. We shall see, in fact, that the application of the supreme law to the solution of the problem of refractions, gives, for its first term, just this fine rule of Bradley. Also it is notorious, astronomers have not, even at the present day, above 12° of altitude, any thing of greater value than this rule.

Meyer, in his Tables of the Moon, returns to the theory of Bradley; but he made a step very great towards the solution of the problem of refractions, by introducing into it the considerations of the barometer and thermometer; an improvement we owe to him principally.

Lambert, in his work on the *Properties of the rout of Light*, &c., published in 1759, and in the Ephemerides of Berlin for 1779, established the principle of a circular trajectory, which, unhappily, is not admissible in general.—Scarcely will it serve for terrestrial refractions.

At length, Kramp conceived the idea of a dynamic theory of refractions, by considering the rout of light as that of a projectile drawn towards the centre of the earth, with a correspondent force. In his *Analysis of Refractions*, &c., he finds easily again, for refractions above 12° of altitude, the rule of the tangent; but, in order to obtain convergent values for horizontal refractions, he is obliged to introduce the algorithm of the *faculties* of Vandermonde, which he appears to have discovered himself, without knowing the latter had used it before him. However, it is true that Kramp was the first geometer, who, precisely in this Analysis of Refractions, has shown all the utility of this algorithm.

About the same time, Oriani, in the Ephemerides of Milan, made the very important remark that, above the altitude of 12°, the construction of the atmosphere, or the law that governs the density of its strata, has not any sensible influence on refractions.

Then La Place, in the fourth volume of his Celestial Mechanics, produced his purely dynamic theory, like that of Kramp; and, renouncing there all of a sudden the algorithm of the series, he was able, as we have already remarked, to obtain, by the introduction of Euler's algorithm of continual fractions, some convergent expressions for all refractions. Unfortunately, this integration is neither general nor specially independent

of all hypothesis; so that, in considering even the problem of refractions under the single point of algorithmic view, it remains still unresolved.

But another consideration, much more important in the solution of the problem of refractions, and which has entirely arrested its progress, is that of the physical construction itself of the atmosphere, that is to say, the consideration of the law which governs the density of the atmospheric strata.—There properly is the principal object of this problem; for, this law being once known, the determination of refractions is reduced to a simple algorithmic question.

Now, all depends notoriously here on the knowledge of three laws, which are:

1st. The mechanic law, that governs the expansion or elasticity of the air, proportionate to the pressure, and which appears conformable to the rule of Boyle or of Mariotte.

2d. The chemical law, that governs this elasticity in proportion to the temperature, and which is not yet fixed, by the rule of Dalton, or that of Gay Lussac, but between the temperatures of the congelation and ebullition of water.

3d. and last. The geologic law, that governs the temperature itself in different elevations of the atmosphere, and which is entirely unknown.

We conceive, by this, that the first consideration of the physical construction of the atmosphere, ought to have been to suppose an uniform

temperature through all its extent, and not to admit, for the establishment of its density, any other law but that of Mariotte, as soon as it was known. Thus, carrying the height of the atmosphere from 32000 to 37000 fathoms, after the estimations of Alhazen, Kepler, Halley, and especially of De La Hire, such as the extent of the twilight gave them, they found, according to this law of Mariotte, that the atmospheric stratum, answering to this elevation, had no more than $\frac{1}{2200}$ of the density of that which touches the surface of the earth.

Euler was the first who introduced, precisely into his Theory of Refractions, an hypothesis which postulated already the three laws on which depends the construction of the atmosphere. Kramp gave, to this hypothesis, a determination which approached it nearer to truth. At length, La Place, assisted more by the force of his genuis, than by the ærostatic observation of Gay Lussac, produced his hypothesis, which, according to what we shall see in the rigorous solution of this problem, presents a strong character of truth.—Recently, Professor Leslie, making the law of the elasticity of atmospheric strata depend on their density alone, gave, in the Supplement to the British Encyclopedia, (vol. 3, article, Climate), a new hypothesis, whose conditions, after what we shall equally view in the rigorous solu-

tion of this problem, present many more difficulties to be verified than those in the hypothesis of La Place.

Whatever may be those considerations, certainly they are only hypotheses; and, in this respect, more than by the defect of algorithmic processes, it is evident that the grand problem of refractions is still found unresolved.

A latter consideration, equally of the first importance in this question of refractions, if not the variation of the chemical state of the atmosphere, proceeding from different gases which may be temporary floating there, is at least the variation of its hygrometric state, which is a permanent affection of the atmosphere. Without ascending further, we will only remark that Kramp sought to introduce it into his theory of refractions. But La Place, cutting the knot, perhaps too hastily, decided that the hygrometric influence was insensible. Nevertheless, Delambre, founded on observations, believed he ought to manifest a different opinion; and, if even the observations of Nova Zembla, made in 1597, should be inadmissible, it could be already averred that the variations of horizontal refractions, caused by the humidity of the air, extend to 4′.

Thus, under this latter aspect, more so than the two preceding, the problem of refractions remains unresolved.

Notwithstanding, the necessity of the solution of this problem is strongly felt in every respect. We will only cite one or two examples.

The determination of the obliquity of the ecliptic by the solstices, notoriously contains an error great as 6″, and which is produced by refractions. And, if we consider the annual variation of this obliquity, which is only 0″. 521; we may conceive the importance of this error.

In about 200 observations of the meridional height of the circumambient polar stars, made by Mr. Pond, at Greenwich, the mean of their distance from the pole, separately deduced above and below this point, and corrected by the table of Bradley, presents a difference that exceeds 1″, at the altitude of about 25°. Mr. Pond, with his ordinary modesty, attributes this error to the imperfection of the actual state of astronomical observations. But, where would be astronomy, if the mean of 200 observations, made with instruments such as those at Greenwich, and by an astronomer so skilful as Mr. Pond, could present an error so great? Is it not more simple to think that the pretended corrections of refractions, especially by the still imperfect applications of the barometer and thermometer, and the exclusion of the hygrometer, have alone caused this error?

But it is so much less necessary to be desirous of proving the want of a rigorous solution of the problem of refractions, as, to this day, practical

astronomy is, in some sort, only a system of artifices, destined to avoid the influence of refractions. Unhappily, these subtilities cannot be always used in nautical observations; and therefore already it becomes urgent, before attempting the perfection of the lunar theory, for to ameliorate the solution of the problem of longitude, to undertake first a rigorous solution of the problem of refractions, in order to give to the observations that accuracy which will be necessary to enable us to introduce these ulterior perfections. In general, we think that the least perfect branch in modern astronomy, is precisely that of the knowledge of refractions; and we are deeply impressed with the truth, that, what prevents the progress of this science, is the defect in an exact knowledge of the general state of this phenomenon.

These are, then, the superior motives which determine us to offer to the Board of Longitude, AS THE SECOND BASE OF ASTRONOMY, the rigorous and absolute solution of the problem of the construction of the atmosphere, and that of refractions.—Here anew, in a few words, are the results which we have obtained.

At first, with respect to what concerns the data, it is sufficient, for this solution, to have some observations of refractions, especially horizontal ones. But these observations ought to be accompanied by all the circumstances of the atmosphere; viz. its barometrical, thermometrical, hygrometrical, and

even anemometrical states. Further, the epocha of the observations ought to be fixed, in order to ascertain the influence of the state of the heavens upon the atmosphere.—Now, in this accomplished state, there exist not yet any observations of refractions. The author, therefore, applied to some members of the Board of Longitude, and directly to the Astronomer Royal, Mr. Pond, to beg him to procure, for this grand purpose, similar observations. This celebrated astronomer received his request with that benevolence which so highly distinguishes the man of true science, in whose breast the welfare of knowledge supersedes every other consideration, and frankly promised the author a transcript of those observations he had already made, and to begin a course of new observations, especially of horizontal refractions, in order to make them answer in all required circumstances. And further, this astronomer had the condescension to say, he would have other instruments constructed, if there needed any, to contribute, as much as was in his power, to the advancement of the science. Thus, in expressing here our gratitude to this worthy philosopher, for this his assistance, we hope soon to be able to undertake the calculation of the tables which should accompany this rigorous solution of the problem of refractions, that we submit to the Board of Longitude.

Afterwards, for that which concerns facts, this solution leads to five superior results, which are the following:

The first result respects the algorithmic part of this resolution of the problem of refractions; and here we are able to announce that this resolution is obtained, not by the means of series, but by a theoretical expression, even rational, developed progressively by the supreme law, and generally applicable to all altitudes, from the zenith to beneath the horizon.

The second result respects the construction of the atmosphere, or the law that follows the density of its strata. Here we want no further hypothesis: the law which we establish for this construction is true in itself. Also, we must observe that it is properly therein that consists the principal character of this solution of the problem of refractions. We ought, therefore, to fix principally the epocha of this discovery, in establishing that it was already announced in 1810, in the aforesaid Report of the Institute of France, and more expressly in 1817, in the note pages 614 and 615 of the second section of the Philosophy of the Techny. — We should here notice a remarkable fact, which is, that, in consequence of this construction of the atmosphere, we have found it is limited in its height; a fact we shall prove by the existence of a new twilight, which

we observed in southern countries, and that results from an interior reflection of the solar rays upon this limit of the atmosphere.

The third result concerns the mechanic modification of the atmosphere, operated by the influence of its chemical state, that is to say, of its thermometrical and hygrometrical dispositions, or generally of whatever state of the air that surrounds us. Here we discover the existence of a principle, forming a general REGULATOR of the atmosphere, and depending upon a single datum. It is there properly this general modification of the atmosphere, which manifests itself clearly enough by the degree of its tension, that is to say, by its barometrical state. But, we bring back this state of the atmosphere to its true and only principle, and in such a manner, that, by this single datum, all the chemical influences, whether thermometrical or hygrometrical, and all others whatever, known or even unknown, are determined by a single circumstance.

But, respecting this barometrical state of the atmosphere, we should here remark that its mechanic construction, or the law of densities being unknown to the present day, the various formulæ which geometers have produced, from De Luc and Trembley, even down to La Place, for the measure of terrestrial elevations by means of the barometer, of which they have recently made such a bustle, have necessarily remained imperfect, or even unexact, as it might be perceived

already by the observations of Ramond. — Now finally, as this construction of the atmosphere is discovered, we can offer, for these barometrical measures, which are really important, formulæ rigorously exact.

The fourth result of the new theory concerns the equilibrium in this mechanic construction of the atmosphere, that we have just fixed by its general regulator; and especially, it concerns the conditions upon which depends the derangement of this equilibrium, constituting the phenomena of *winds*. Here then, having at length thrown light upon the true construction of the atmosphere, we can discover the conditions of the derangement of its mechanic equilibrium; and, pursuing these conditions, we may determine, for every given place upon the surface of the earth, not only the periodical winds, such as the trade winds and monsoons, but generally the prevailing winds in this quarter. Thus, although it may be impossible to predict daily, for every given place, the various winds that may succeed there, because they depend in principle upon a meteorologic or foreign influence on the earth, the science will be competent, even at present, to fix, with a sufficient exactitude, the anemometric state of every place of the earth, that is to say, the sum of its dominant winds, and their periodical return; a knowledge, which, in the social economy, is notoriously become of the highest importance.

At last, the fifth result of this absolute theory concerns the change in the state of our atmosphere by the progress of ages, that is to say, its archeologic state. And if, unhappily, we shall not here be able to recover any trace of the ancient state of the atmosphere, we can at least, by the means of our theory, fix this fleeting element, wherein no vestige remains; and we may thus transmit to posterity a positive knowledge of the actual state of the atmosphere, with all the details of its construction. We shall bequeath thus to generations unborn, the means to ascertain all the variable and periodical alterations of this subtle element, which appears to be not only the vital principle on our globe,—but much more, the principle of all terrestrial generation. Perhaps even we shall be able already, in the lapse of a certain number of years, to discover some like archeologic or periodical change, at least in its grosser manifestation, corresponding to an alteration of seasons; and this knowledge would not fail to reflect a brilliant light on the developement of geology in general.

Such are then the fruitful results that this new theory offers us, as much in general for the construction of the atmosphere, as in particular for the determination of refractions.—If any member of the Board of Longitude would be willing to undertake, conjointly with ourselves, or even alone, the construction of these tables of the

atmosphere and refractions, we would eagerly transmit to him this theory, and consult with him on the best mode for the construction of the same. Then we should be able to undertake this labour upon that magnificent scale, that the Board of Longitude might present to Europe a system of tables for the state of the atmosphere and general state of refractions, such as she could scarcely have expected, and probably such as would continue for ever.

In terminating here our scientific exposition, it is with profound grief we find ourselves compelled to quit science, and claim the attention of the Board upon those adverse conditions of humanity, which often cause the destruction of the fairest fruits of labour. But we console ourselves by this reflection, that the welfare of knowledge is the only motive that directs us. Why have we not another fortune to sacrifice for the production of our labours? We would then freely offer them to the British Board of Longitude, like our other mathematical productions, completely published, and without feeling any other expectation but that of good, which is their object. Unhappily, it is now beyond our power to realize those generous sentiments, which have animated us during so great a portion of our life. It is the farther removed from our power since we have touched the soil of England, where a fatal misunderstanding, unexampled in the learned world, has preci-

pitated us into a position to which, perhaps for the benefit of science, we are, at length, compelled to draw the attention of the illustrious members of the Board of Longitude.

Having finished our new celestial mechanics, and established the two grand warranties of their completion, which we have just made known, and having, by that time, sacrificed a very considerable fortune in the publication of the new principles of the mathematics, we were reduced to profit from the authentic offer made by the British Parliament to reward useful nautical and astronomical discoveries, especially those which relate to the problem of longitude. Thus, relying fully, on one part, in this act of the English Legislature, and, on the other, in the certainty attached to our mathematical productions, we conceived the just hope of finding, in this generous and legal offer of the British Nation, as a reward for our labour, the means of publishing the numerous results we had obtained under the new point of prospect in which, by our toils, we had placed the science.

Our celestial mechanics, and especially the lunar theory, destined to resolve the problem of longitude, and the two astronomical bases we offer as anticipated pledges of the truth of our labours, that is to say, the construction of the earth, and that of the atmosphere, are so plain, that a simple scientific examination, without ulte-

rior experience, is sufficient to effect the acknowledgment of their infallible truths. In fact, the object of these different theories are given by nature; and the question is no more, in these theories, than to develope, by rigorous mathematical calculations, the declared incontestible principles of these objects of nature. But, unfortunately, it is not the same with the new micrometric theory, which presents the ready means for determining the structure of the earth. Realized entirely by instruments of human fabrication, this theory can only be proved by the instruments themselves. Thus, desirous of carrying to the Board of Longitude securities quite finished, and reposing in the unimpeached faith of the British Parliament, we sacrificed the remainder of all the property we possessed, in the construction of this system of micrometric instruments, and the making previous experiments on vision, in order to enable us to establish the principles of these instruments. This voluntary sacrifice to the Board of Longitude is incontestible; for, as a simple individual, the author could not derive himself any advantage from a system of instruments destined for the measure of the globe, which governments alone are able to employ.*

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* We remit here subjoined, to the Secretary of the Board of Longitude, the authentic documents which prove that, in the seven months previous to our departure for

Thus, furnished with the new Lunar Theory, and the two previous warranties that we brought for our Celestial Mechanics, and which we confirmed by a great sacrifice, we presented ourselves to the Board of Longitude, by means of their Secretary, Dr. T. Young, to whom we made known these different results.

From the first moment we encountered that indifference, we may say even want of attention, so strongly marked, that we knew not how to conciliate these proceedings with the incontestible importance of the scientific results which we presented to the Board. Our disappointment was extreme; for, it was not in the power of man to render false that which is true, and it was true we had brought to Great Britain the accomplishment of the grand institution of her Board of Longitude.

England, during which time this system of instruments was constructing, we had pledged at Paris property to the amount of £3000 sterling, all we possessed, for to procure us the sum of about £700 sterling, which was wanting for the completion of our system of instruments.

The object we propose here to ourselves, in giving this authentic proof to the Board of Longitude, is not to demand, either directly or indirectly, under whatever pretext, any indemnity.—Our only object is to prove, by this great sacrifice of our *all*, the importance we attach to this micrometric system, with whose scientific worth we ought to be well acquainted.

We will not descend here to relate these human miseries. We will confine ourselves to the saying, that, in our misconception, not being able to conceive this absence of all interest for such great and useful scientific results, such at least as they are in our own conviction, it was natural to suppose some personal prejudice. And, unhappily, many appearances, false no doubt, seemed to confirm our supposition. Thus, having direct relations only with Dr. Young, the Secretary to the Board of Longitude, we committed an error by thinking that, in consequence of a mistaken depreciation of our labours, the Secretary thought it would be serving the Board in using his endeavours to send us back to the Continent. We anticipated then this desire we supposed in the Secretary, and prepared ourselves immediately to quit England.

Unfortunately, a mistake of more serious consequence prevented our resolution, and increased to its utmost point our error. It not being possible for us, at any time, to meet together Sir Joseph Banks and Dr. Young, we could not, as it appears, make ourselves clearly understood; for, contrary to our well pronounced intentions, an order was demanded by Sir Joseph Banks, and given to the Custom House, that our instruments, which were detained there, should be sent to the Board of Longitude. This step excited, at least, our surprise: we could not reconcile that indif-

ference, which had been unceasingly expressed towards us, with that lively eagerness, they now felt to procure a sight of our instruments.

This proceeding, which we cannot impute to the Board of Longitude itself, was so much the more grievous for us, as not having been able to produce our labours before this Board, and seeing ourselves compelled to return to the Continent, it behoved us to preserve these instruments untouched, the hard fruits of our sweat, that we might be then able to present them to some other Admiralty of the nations of Europe, and particularly to that of our Sovereign, the Emperor Alexander.

But a much greater misfortune rushed upon us through our first fatal mistake, which was, that, by the detention of our instruments, we were, in a manner, forcibly kept in London, beyond the period our pecuniary resources warranted. Further, when we complained of the same to the Secretary, we believed to understand from Dr. Young a promise given in the name of the Board, that our expences should be paid us until the time of the restoration of our instruments; and, under this erroneous opinion, we contracted a debt for our subsistence, nor were undeceived of our gross error but upon the receipt of the instruments, and when we were not in a condition to discharge what we owed.

We cannot refrain here from making a remark, sufficiently impressive to cause that situation to be

felt into which we were cast, through the above fatal mistake. It is this: if it be true, what we must now no longer doubt, as they maintained on the restoring us our instruments, that they had made us no such promise of the payment of our expences, during the time they detained them; of course, no other alternative remained to us but to quit England immediately, and abandon, altogether, these instruments to the Board of Longitude. And if, on the contrary, it were true, a circumstance we strongly believed at the time, though now unhappily undeceived, that they really promised to pay us our expences, it may easily then be conceived what our sentiments were, when, upon their rendering our instruments, it was pretended they had made us no such a promise.

Thus, a learned stranger sacrifices his property, and a large portion of his life, to enable him to reply to the public call of the British Parliament. He obtains results far beyond what the Legislature demands. He dissipates the remains of his fortune in the construction of a system of instruments, necessary to draw instant utility from one part of his great scientific results. He repairs to England with these his only treasures, full of confidence in her unimpeachable faith, and believing those results would have been deemed precious by the nation, after the appeal of her Parliament to the world. He brings to the Board of Longitude the full accomplishment of its institution. He is there

received with coldness; nay, he is counselled to return to the Continent, without their intimating the least wish to enter into an examination of his labours, though publicly constituted for these purposes. Nevertheless, in opposition to his express desire, they obtain a view of his system of instruments. By this proceeding, he ceases being master of what had cost him all that he possessed; and is thus reduced, either to leave England, and abandon his instruments to the Board, in order to avoid the contraction of debts, or remain, and repose on delusive promises*. They restore, at

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* In the Report of the Board of Longitude to the Government, which is the subject of the Advice that precedes this Address, Dr. Young, Secretary to the Board, speaks, with a species of generosity, of pretended pecuniary succours, which he says have been given to the author by the members of this Board. If it had been in our power to have imagined this generosity, we should have been eager to have expressed our gratitude to the illustrious Board; and should thus have spared the Secretary that effort he was obliged to make upon his delicacy on revealing benefits. Unhappily, entangled by our general error, we could not view, in these moderate pecuniary advances, any other thing but the means of placing us under the legal impossibility of opposing the extraction of our instruments from the Custom House, where they had been deposited. In fact, eight days after his arrival in London, it was agreed by the members of the Board of Longitude, that the author should return to the Con-

length, to him his instruments, but their promises are not realized. He is then, contrary to his in-

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tinent, and take back with him all the labours that he had brought to the Board, and namely the instruments, whose cases still bore the seals of the French Custom House, when the Secretary, out of respect to the useless expences of the author's journey, offered him, by his own act, the sum of ten pounds sterling, under the title of a loan, in causing him to deliver a receipt, and gave him a letter to Sir Joseph Banks, who was to advance him, under a like title, a further sum of twenty pounds. The farewell was mutually exchanged the same day; but, on the morrow, an order was demanded for the Custom House to convey the instruments to the Board of Longitude. We will limit ourselves here to the alleging a single proof of the extreme facility where we were to fall into error under all these appearances: it is the letter which Dr. Young wrote us the 23d February, in which, after announcing this order given to the Custom House, he tells us expressly: "*As soon as I* RECEIVE *the instruments, you may* DEPEND *on 20l. from Sir Joseph Banks;*" and he himself underlined these two words, *receive* and *depend*. This letter at once appeared to us to prove incontestibly two things: the first, that the pecuniary advances which had been offered to the author, were not acts of generosity, as the Secretary now has informed us; and the second, that it was very well understood by the Board the author had refused to produce his instruments, since they were thus desirous of purchasing, for a few pounds, this precious production. This last circumstance appeared to us, even at the beginning, so marked, that, perceiving the impossibility to oppose it, and having no more in our power but the desire of prompt restitution

tentions, entangled in debts, and loses all he brought to England. He is thus subjected to physical pri-

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of our instruments, we believed we ought to shew an extreme condescension or rather complaisance to these procedures, so contrary to our interests; and this we did in our letter to the Secretary, of the 18th of February, whose sole object was manifestly but to accelerate the return of these instruments. At last, to legitimate, equally, the error into which we had fallen on account of the promise they made us, whilst they were desirous of procuring thus a view of our instruments, to defray our expences in London, during the detention of the same, we will say that, upon the restitution of these instruments, they gave to the author a last sum of ten pounds, as the full exchange for all promises.

Thus, a sum of forty pounds was, in this manner, advanced to the author; and since, as they now tell him, it was an act of generosity, the author will not remain behind in expressing here publicly his gratitude, by beseeching the members of the Board of Longitude to accept the excuses he has just alleged for not having done it before he was reminded of his task. However, he may hope to obtain pardon for this delay the more easily, as the illustrious members of the Board of Longitude will deign to observe that the author could with difficulty perceive his obligations for this sum of 40*l.*, at the moment when he had just sacrificed *three thousand pounds sterling* for the Board, what is incontestibly proved by the documents which accompany this Address, without speaking here of sacrifices far greater, that he was obliged to make, during a long course of years, in order to enable him to answer worthily to the appeal of the British Parliament.

vations that injure his health, and is exposed to painful humiliations from men with whom he is reduced to live. Being not able to discharge his debt, he is overwhelmed by the just resentment of his creditor; and, to complete his misfortunes, not only his personal liberty, but even his precious manuscripts, the labour of his life, are delivered up to the mercy of this creditor, who, happily, is not so barbarous as those who precipitated him into these miseries.—Such are a learned stranger's rewards for his vast sacrifices to the enlightened, generous, and brave British nation.

There is, my Lords and Gentlemen, the extreme position into which I was plunged by a serious error, two months after my arrival in the country of Newton. You will excuse, without doubt, my error, on reflecting that the mistake of Sir Joseph Banks, respecting my instruments, caused appearances more than sufficient for such a delusion. And if, my Lords and Gentlemen, you further compassionate the grief I must have proved, when sensible of my situation, you will the more readily excuse all the steps I have taken, since the explosion of my error, to obtain at least the payment of that debt I was compelled to contract through the fatal misconception.

You recollect, my Lords and Gentlemen, that, even at the commencement of this prolonged course of misfortunes, when my forced debt was yet very small (about 50*l.* sterl.), I presented you a new

theory of astronomical refractions, entirely finished, and accompanied with detailed instructions for the mechanical calculation of the tables of this phenomenon, in beseeching you if, according to the Article VIII of your statute, you should judge this production worthy of a reward, to deign to grant me, at least, that moderate sum necessary to discharge this debt, of which you were, without knowing it, the true authors. You returned me, my Lords and Gentlemen, eight days after the receipt of them, these important results, without deigning absolutely to take the least notice of them, as was expressly announced to me by your Secretary, in his letter of the 27th of April, in which he says: "*the Board cannot accept of any of your proposals.*" Unhappily, to provide myself against certain scientific incompetence, that I believed was demonstrated to me, I had preceded my theory with this supposed demonstration, in declining thus the judgment of one alone of your members. I could not suspect that this exception, purely individual, which besides was consented to by this same member, in his letter of the 17th of April, should have paralised the whole Board.

Many of your members, my Lords and Gentlemen, ought equally to recollect the importunities that I have unceasingly made to them, since these disastrous events, in seeking, by all the possible channels, to open a way to render serviceable such immense scientific results, at least to merit the

small sum necessary to discharge my debt. I would willingly have sacrificed all that I had brought, instruments, books, &c.: I sought but to leave England freely, where, for the first time of my life, I observed the little worth of this property the most costly to humanity, that is to say, of the TRUTH revealed by grand scientific discoveries.—Those members that I have thus oppressed with my importunities, should recollect, that they themselves counselled me to quit their country, where they told me it would not be possible in me to set a value on my labours. Unfortunately, an impossibility, both moral and physical, opposed itself to our mutual wishes, and still increased daily.

But the deepest and most painful of my sufferings was this—to behold such a mass of labours, so many results, as happy as important, meet a fate so undeserving the great institution of the British Board of Longitude, and of that illustrious nation so famed in science, under whose protection this misfortune happened. I did not conceive that either the learned members of the Board of Longitude, or the enlightened men of the empire could see, with indifference, this heavy calamity to science, which, if my announcements were true, could not be so soon repaired. But, in the midst of my grief, I have these consoling reflections, that I have never conceived any injurious idea, either against the body of the Board, or the nation in whose bosom I have been so completely over-

whelmed. I have given positive proofs of this truth by the confidence I have placed in both, in my *Appeal to the British Parliament*, wherein I have demanded my judges from the Board of Longitude itself, and reposed all my security in the nation's honour.

Happily, at the moment of my going to carry, in all its forms, to the British Parliament, this humble Appeal, I had time to acknowledge the error I had incurred by reckoning on a better success. I perceived clearly, after holding conversations with many intelligent persons, that, on my first arrival in London, I had not succeeded in giving to the Board absolutely any idea of my labours.

Several learned characters put questions to me, which one should have supposed they would scarcely have ventured in the ancient obscure state of the mathematics. Not one appeared to have conceived an idea of the reform of the science, or of the absolute state to which it had just been brought. I was surprized to hear men, and even learned men, in a country that, if it did not give birth to the mathematics, at least greatly improved them, treat the questions of celestial mechanics and others as if they still depended on the ancient state of the science. Those who, in consequence of much conversation, began just to glimpse the new point of view, dreaded, as much as to say, the excess of light, and hastily retreated into

the ancient obscurity. They feared, in some way, that these absolute methods might be too mighty; for, I cannot otherwise explain that dread which they manifested upon the application of these methods to ordinary physical questions. Could they suppose the mere outlines of the science, the coarse and, in some sort, elementary forms of the mathematics sufficiently powerful for the solution of these questions; and the perfect science herself, with her superior absolute methods, too feeble for such ordinary applications? They did not reflect that these common or elementary methods are, in themselves, but very particular cases of these superior or absolute laws; they did not think that when they are desirous of clinging to the former, they only act by means of the latter, stedfastly march under their colours, and can, by no means, run counter to their rule. In fact, all that has been done in the mathematics, and all that shall be possible to be done in them, no matter by what means, processes, or methods; all has been, and all eternally must be, but a particular case of these supreme methods.

Those geometers who have already fathomed, at least in part, the reform in question, certainly will not express any doubt upon the positive assertion I have just advanced, and which is as decisive for the history of the progress of our knowledge, as it is important for the science itself. It can only be with those mathematicians who have not re-

ceived, as yet, any idea of this new state of the modern science, that contrary manifestations could be ventured. And as for these latter, we may, by anticipation of their study of this reform, merit from them some reservation by the following considerations:—

First, for all that which has already been done in the modern mathematics, we can allege, with a kind of European authority, the opinion of the Institute of France, which further is sanctioned by the imposing name of La Grange. This authority, almost incontestibly, proves that all the modern mathematics are contained in our supreme law, which law forms that grand base of the reform spoken of. There cannot, therefore, be any pretext for doubt, even in those who do not know this reform, but by report to the future state of the science.—Here, nevertheless, is a mean to merit a favourable opinion for this decisive future perfection in the science; by shewing that the different methods, which geometers have produced in the mathematics, since the publication of the reform in question, are effectually nothing else but *very particular cases* of this new system of the science. Unfortunately, the extent of this Address I have the honour of presenting to the illustrious members of the Board of Longitude, will not allow me here to enter into an examination of all those mathematical labours, produced in the learned world since the time we mention. But, perhaps, it may be

sufficient, in this Address, to characterise, at least, those labours in the science done in England, particularly those which enjoy an authentic reputation by their insertion in the Philosophical Transactions of the Royal Society.

I ought then, in order to merit this favourable opinion for the future of the science, to show that all the new methods which have been produced in the Philosophical Transactions since 1817, the time in which the publication of the reform of the science was finished, are really but particular cases of the absolute system of methods which constitute the new state we have founded.

The first memoir that presents itself is that of J. Herschel, Esq. (Trans. Philos. 1818; Part I, No. ix.) entitled: "*On circulating functions, and on the integration of a class of Equations of finite differences, into which they enter as co-efficients.*" Now, the integration of this class of equations, that this philosopher obtains in a manner quite ingenious, is only a particular case of the theory of functions *Aleph*, resulting from our reform; a theory which, already in the Introduction to the Philosophy of the Mathematics, is fixed in its foundations, viz. at page 145, as principle of the recurrent series, and, at pages 143 and 144, as principle for the integration of equations of differences. In fact, whatever may be the co-efficients a, b, c, . . . k, of Mr. J. Herschel, constant or variable, the general theorem (de)^{n}, which is found

at page 144 before cited, will always give, as a very particular case, the integration of equations this learned gentleman has himself proposed.*

The second memoir which presents itself, is that of C. Babbage, Esq., (Trans. Philos. for 1819; Part II., No. xviii.) entitled: "*On some New Methods of investigating the Sums of several Classes of infinite Series.*"—Now, this method is nothing more than a particular case of the *Algorithmic Canon*, such as is found fixed in the first section of the Philosophy of the Techny, at pages 142—153. But Mr. Babbage limits himself to the result $(45)^{iv}$, and does not introduce even the developement $(45)''$; which circumstance contracts much his method, and constitutes it but a very particular case of this Algorithmic Canon. At the same time, the method of this philosopher proves, by the fact, that the Algorithmic Canon extends itself far beyond the calculation of the fonctions generatrices of La Place, which is also but a particular case of

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* If Mr. Herschel would desire to know all the details of this integration, we would with much pleasure communicate them, adding a request that this Gentleman would then present the same to the Royal Society, of whose illustrious body he is a member. Therein properly consists the general integration of equations which is announced at the end of our Critique on La Place, and of which the various integrations contained in this Critique, are so many particular cases.

this Canon, and differs essentially from the fine method of Mr. Babbage.

The third memoir is that of W. G. Horner, Esq., (Trans. Philos. for 1819; Part II. No. xxi.) entitled: "A *new Method of solving numerical Equations of all Orders, by continuous Approximation.*" Now, the principles postulated by this philosopher, are those of the calculatiun of derivations by Arbogast; and, unhappily, we have already proved, in the second section of the Philosophy of the Techny, that these principles, purely artificial, ought to be rejected from the science. But, to confine ourselves to their results, we will observe that the theorem Mr. Horner alleges at No. 7 in his memoir, and which is the principle of his method, though it may be ingenious, is only a very particular case of the theorem which is found under the mark (232) of the Philosophy of the Techny (2 section, page 111). If Mr. Horner had followed this general theorem, resulting from our reform, he might have got out of the circle of mere trying, and rose to the direct method which results from this theorem under the marks (232)″ and $(232)^{iv}$. (Idem, pages 112 and 113.)

At last, the fourth memoir, concerning new mathematical methods, has been promised in the Nautical Almanac for the year 1822, as being to be inserted in the Philosophical Transactions for 1819. It is from Dr. Young, Secretary of the Board of Longitude and of the Royal Society;

but this memoir, which has for its object astronomical refractions, and according to which has been calculated the Table of Refractions in the Nautical Almanac above cited, is not to be found in the Philosophical Transactions. Nevertheless, as it has been published separately, under the title, "*Postscript to Dr. Young's Letter on the Reduction of Experiments; No. 5, Corrections for Refractions,*" we shall here speak of it.—Now, Dr. Young, to arrive at the integration of his differential equation, introduces a series whose progressive powers are those of the unknown quantity. This is no other thing but a direction towards the neutral algorithmic generation, which, as one of the results of the reform in question, is found fixed at the end of the second section of the Philosophy of the Techny; but, instead of a single like series, two of them are wanted to arrive at this superior degree of the algorithmic generation. Also Dr. Young falls into the resolution of equations of superior degrees, and consequently into irrational quantities, which are here entirely useless. Unhappily, besides this imperfection of the neutral generation, towards which Dr. Young has probably reached without intending it, there is slipt, into his memoir, a small error that renders his results entirely inaccurate; as he has been forward to acknowledge himself, in one of his letters he did me the honour to address to me, where this confession is made in

the terms reproduced in the Advice that is prefixed to this Address. Perhaps it is this very error that has prevented Dr. Young from introducing his memoir into the Philosophical Transactions, according to his announcement. In this case, his superior consideration for the truth will reflect still more honour on this philosopher than his idea of the neutral generation, which he manifests indirectly in the memoir in question.

Thus, it is averred, that all the new mathematical methods which are produced, and which were to be produced, in the Philosophical Transactions of the Royal Society, since the publication has been completed of the reform of the mathematics, are effectually but very particular cases of the superior methods that this reform has fixed for the future state of the science.—We may then, with renewed confidence, repeat here, that all which has been done in the mathematics, and all which shall be done in them, no matter by what means, processes, or methods, all has only been, and all will ever be, but a particular case of these superior methods.

But, to return here to the misfortune I experienced through my not having given, upon my arrival in London, a just idea of my scientific results, I will observe, that, what seemed the most remarkable in the various opinions which were formed of my labours, was the recommending to me the doctrines of Locke for the deduction of

philosophical principles, as if, for a long time past, the scepticism of Hume had not already done complete justice to this gross sensualism, even in the Philosophical Schools of England.

It thus became manifest they had not conceived the least idea of my works in general, and, only to speak here of the positive mathematical results which I brought to the Board of Longitude, it also proved clear they did not even know what the question meant, so far these results ascended beyond the sphere of the received ideas.

I then perceived the full extent of my error, and felt that the cause of this bad success, and source of severe misfortunes I had experienced in England, ought to be attributed to myself, in not having made known, especially by some positive fact, the nature of the works I brought to the Board of Longitude. The reparation was easy, without doubt: it was only to present, at last, to this illustrious body, one of the grand applications of the new Celestial Mechanics; as, for example, the absolute solution of the problem concerning the construction of heavenly bodies, and especially that of the earth, such as we have here subjoined.

But, unhappily, it was now too late: the copy of this manuscript exacted, at least, the hard labour of four months, and already, during five months of previous residence in London, seeking vainly the means to discharge my forced debt, I

had been drained of all my pecuniary resources. Of all that remained to me out of the sacrifice I had made of the residue of my fortune in the construction of the new system of instruments, was a small library provisionally left by me at Calais, on my route to London. This last fragment of all that I possessed, was now equally engulphed, in being transferred as a security, at least, of a part of my debt. There remained thus for me no means to drag on my existence in London, during the four months ponderous labour, which it required me to make a copy of my manuscript, that I was to present to the Board of Longitude, and upon which I had now a right to found fairer hopes.

Happily, a lady, at whose house I resided since my arrival in London, and to whom precisely I owe the expence of my subsistence during all that time; this lady, I say, consented, through pure humanity, to give me an asylum until the completion of this labour. I repeat, through pure humanity; for, being abandoned from all the learned with whom I have had relations, suffering the total failure of all success, and discovering the misfortune into which I was plunged, in consequence of the detention of my instruments, I could not impress this lady with ideas favourable to the objects of my coming to England. Nor was it in my power to give her hopes even by this last positive labour: interest, therefore, could clearly have

had no impulse in this kind conduct; it was the noble inspiration alone of humanity. It becomes, therefore, my duty, as a debt of gratitude, due to this generous woman, to declare publicly, that if any benefit should result to science from this my last effort, the world will owe it to this lady, Mrs. Thiot.

Thus, I can now, my Lords and Gentlemen, present to you this first Base of Astronomy, or rigorous solution of the great problem of the construction of the earth, as a perspective of that application of absolute methods to the celestial mechanics, from which results the new lunar theory, giving the definitive solution of the problem of longitude.

But, my Lords and Gentlemen, in conformity to the reasons I have previously adduced in this my Address, will you permit me, perhaps for the welfare of science, to beseech you, in order to render possible the ulterior production of my labours, to condescend to attach your consideration to the train of my hopes, which is subjoined:

1st. I have the honour of presenting, to the Board of Longitude, this first Base of Astronomy, in the first place, to reply to the appeal of the 8th article of the last statute, enacted for the direction of this Board, that is to say, as a *simple proposal, useful to science.**

* Anno 58. Georgii III. Regis, Cap. XX; viii. — And whereas it may happen that Proposals, Inventions, and Ta-

2nd. If the Board of Longitude shall think it right to approve this work, I could then form the hope of submitting to them the new micrometric theory, and system of instruments, resulting from it, destined to measure the curvature of terrestrial arcs, for to realise, the soonest possible, the absolute determination of the form and structure of our globe.*

3d. If the Board of Longitude may judge it convenient to examine this micrometric theory, and geodesic instruments, before pronouncing

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bles, or Corrections and Amendments of former Proposals, Inventions, or Tables, ingenious in themselves and useful to Science, and which may deserve Encouragement, though they do not come within the Limits and Conditions specified for the before-mentioned Rewards, may be made to the said Commissioners: be it therefore enacted, that the said Commissioners may pay or cause to be paid such Sum not exceeding Five Hundred Pounds to any one Person for any one Proposal or Invention, or Two Thousand Pounds in one Year, as they may consider the said Proposals, Inventions, Tables, or Corrections to deserve.

* The Board have already seen the instruments, and some of their processes, especially the Marine-Teleometer, but have not, by any means, verified them. The Board, therefore, cannot know their scientific value.—They have also seen a perspective of the application of the micrometric theory to topography, but have not yet seen the theory itself, nor especially its application to the determining of the curvature of terrestrial arcs. They cannot, therefore, know as yet, either the scientific value of this theory, or its application to the measure of the earth.

their final decision on the present work, although these two things may be considered independent, I will hasten to conform to the views of the Board, and submit immediately to them these micrometric productions.

4th. As soon as the tables of the atmosphere, and of the refractions, are calculated, I shall then have the honour to present, to the Board of Longitude, the second Base of Astronomy, or the rigorous solution of the problem of the construction of the atmosphere, and of the determination of refractions.*

5th. In case the Board should desire to know this absolute theory of refractions, before the calculation of the tables, I am prepared to submit it to them.

6th. If I am so fortunate that the Board of Longitude should allow these two Bases of Astronomy, the construction of the earth, and that

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* The Board have already seen the analytic part of this solution, where they might have perceived the manner of determining the law of densities, without the need of any hypothesis; and the integration of the fluxional equation by means of the algorithmic neutral generation, resulting from the combination of two series.—But they have not yet seen the absolute part of this solution of the problem of refractions, which is naturally the true solution of this problem, and such as we offer to the Board of Longitude.

of the atmosphere, as simple *proposals useful to science*, conformable to the eighth article of their statute, I shall then pray the Board to be willing to consider these two productions, as the two anticipated warranties of an absolute lunar theory.

7th. And if the Board effectually find in them like pledges to enable the learned members to found a hope of an absolute state in the lunar theory, and consequently in the solution of the problem of longitude, I will then take the liberty of praying the Board to exert the faculty, with which it is invested, by the 5th article of its new statute, to wit: of proposing to his Majesty the establishment of a reward, such as the Board of Longitude shall deem proportionate to this absolute state of the solution of the problem of longitude.*—Immediately as I shall be thus authorised, I will hasten to submit to the Board this absolute theory of the motions of the moon,

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* Anno. 58. Georgii III. Regis, Cap. XX.—And whereas it is expedient that such Limits and Conditions should not be immutably fixed by Act of Parliament, but should be regulated on scientific Principles by the said Commissioners for the Discovery of the Longitude, and should be varied from Time to Time according to the Progress of Discoveries and the Advancement of Science: Be it enacted, that the said last-mentioned Commissioners shall from Time to Time, as they may see proper, propose, by their Memorial to His Majesty in Council, to direct and establish Three Scales of proportionate Rewards....

which, if I am not deceived, offers the full accomplishment of the institution of the British Board of Longitude.

Such are, in summary, my Lords and Gentlemen, the hopes I form, before you shall have decided otherwise upon them, to render possible the production of my labours, which I have the honor of submitting to the Board, beseeching you, if you deem them worthy of your examination, to deign to take them into your immediate consideration.

But these hopes I can only cherish distant from obstacles that will infallibly present themselves; for such, by misfortune, is the condition of humanity, that the good the most precious, which she appears to strive after as the term of her existence, — in a word, that TRUTH she constantly seeks, ever meets obstacle on obstacle, and cannot establish itself but upon the ruin of those barriers opposed to it. Nevertheless, one of my hopes will certainly realise itself; which is, that, sooner or later, these truths I present to the British Board of Longitude, will terminate in the triumph of their establishment; for, such also is, by a species of compensation, the slow, but perfect, march of humanity, at least, while she continues to pursue her high destinies. All I can do, as a simple individual, to hasten the approach of this infallible term, is to signalise those of the ob-

stacles I can foresee, and make every personal sacrifice to endeavour to remove them. Your power, my Lords and Gentlemen, is more extensive; and what concerns here your duty, it does not become me to dwell on this topic.

Two of these obstacles glare, even now, distinctly on my view. And, unhappily for me, they originate in myself.

At first, led by the error which predominated in my disappointment, until the commencement of the present work, I attributed to Dr. Young a personal prejudice against me, and had the misfortune to complain publicly of it, but not in the way of accusing Dr. Young, who might have acted under a sense of duty towards the Board; but that blind fate, which, at the commencement of my intercourse with the Secretary, as if purposely, raised this insurmountable obstacle. Happily, towards the term when I learned the cause of my disappointment, I learned equally my error with respect to Dr. Young. A Member of the Royal Society, in the presence of the distinguished Mr. D. Gilbert*, Member of Parliament, and

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* The author thinks it right here to express to D. Gilbert, Esq., his grateful acknowledgements for the very enlightened manner in which he has been pleased to regard his labours, from the commencement of his relations

one of your illustrious Institution, kindly informed me that Dr. Young, far from seeking to depreciate my labours, was pleased to attach to them a value, which his benevolence certainly exaggerated. I ought, therefore, here publicly to repair the mischief which this error caused to myself; and I do think, that now, when I abandon the judgment of my works, without reserve, to all the members of the Board of Longitude, there cannot arise further obstacles from any resentment whatever, as in my own opinion none can exist.

A second prevention, but which, perhaps, may be only imaginary, appears to me to result from that absolute retreat, which I forced upon myself in order to accomplish this enterprise so vast, the reform of the science. In fact, it was necessary in me to renounce all political distinctions, whose splendid career I followed, and that greater honor of participating with the learned bodies, from which, by my resignation, I totally abstracted myself, that I might surrender myself entirely,

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with the Board, and particularly for those flattering expressions in this Gentleman's letter to the author, dated Oxford, wherein he conveys this high personal compliment—"*I profess to have formed an high opinion of your mathematical attainments.*"

for a number of years, to this single important object. Thus, I can neither display high honorable titles, nor those still more precious, of being a member of the learned bodies of Europe.—No, my Lords and Gentlemen, I only arrive with the simple truth, quite undisguised, for the protection of which precisely are instituted both, the philosophical and political societies. This humble title, will it be sufficient to recommend my labours to the powerful protection of the British Board of Longitude?

There, my Lords and Gentlemen, is the question I have had the honour of submitting to your decision, from the beginning of my Address, and which I believe I have, at length, completely developed.

Whatever may be this your decision, which henceforth I can expect with calmness, I owe, as much to the British Board of Longitude, as to Europe and Posterity, the sacred obligation to deliver a faithful and exact account of this Address. Permit me then to add, I feel perfectly the value of all the expressions therein used, and consequently suffer from that necessity, which my position imposed upon me, of announcing myself the importance of the truths which chance has caused me to discover. But I was compelled to the heavy grief of revealing all, in order that nothing should remain unknown. Thus, I find

myself under the shelter of a still heavier grief, which may result from an high responsibility. I know, that, before the Illustrious Board, whom I address, as before Europe and Posterity, I shall find no excuse for having yielded to this painful necessity, but under this single condition, that the resolution of the Problem of the Construction of the Earth, which I give as the present warranty of my labours, is really ABSOLUTE, or such that man will never have another, nor even demand another.

MY LORDS, AND GENTLEMEN,

With the most profound respect, I have the honor to be

Your very humble

And obedient servant,

HOENE WRONSKI.

London, Dec. 1820.

POSTSCRIPT.

After our conclusion of this work, and the Address which we have just had the honour to expose to the members of the Board of Longitude, we have judged it necessary, in order completely to fulfil this Theory of the Construction of the Earth, to join to it, even now, its most important corollary, the *Theory of Tides*.—We offer it as a security, in some way material, for this—that the laws which we here give for the formation of celestial globes, are really absolute, by shewing that this theory so difficult of tides, and for which there does not exist, to the present day, any thing peremptory, is but a very particular case of these same laws, which govern the construction of our globe. We think, with respect to this theory, as to that of the whole formation of the earth, that this famous problem of the flux and reflux of the sea is, at last, resolved in an absolute manner.

The results of this absolute solution are, that, from the present day, all the fundamental circumstances of tides, such as their height, the position

of their culminating points, polar depression, the time of their formation, and even all the local anomalies, dependent on the situation of ports or maritime coasts, such as the hour of high water, delay, depth, &c., all these circumstances may be calculated, with facility and exactness, for any time whatever given, and at any maritime place known on our globe. Thus, all the circumstances of the phenomenon of tides, whether general or particular, may at present be predicted, with the same precision, as all the other celestial phenomena. In consequence, we take the liberty of proposing immediately, to the Board of Longitude, a mode for these calculations, in such manner that henceforth they might annually be made and announced, as *Tide Tables*, in the *Nautical Almanac*.

In speaking here of the annual calculations of tides in the Ephemerides, we ought to advise that those, which the French Board of Longitude produce in their *Connaissances des Temps*, on a comparison with the positive results, that the absolute theory gives, are found to be not exact. But the better to mark this inexactitude, and generally for the better characterizing the new theory, we should here, in a few words, fix the true state of the problem of the flux and reflux of the sea.

We will not ascend unto the Greeks, who, in consequence of their geographic position, could be but little instructed in the phenomenon of tides. Every one knows the astonishment expressed by

Alexander, the conqueror of Asia, on his perceiving, in the Indian seas, the flux and reflux of the waters.—Pytheas, according to Plutarch, is the only Grecian who, during his stay in Great Britain, perceived the agreement of the tides with the lunar motions.

We will neither go back to the Romans, although these, by the extent of their conquests, were certainly better instructed in the phenomenon of the flux and reflux of the ocean; as may be seen in Cæsar's Commentaries,—in Strabo, who follows Possidonius,—in Seneca, and especially in Macrobius. But, we must observe here, that Pliny, besides his having already signalized the influence of the sun, appears also to have conceived the true cause of tides.

We will still less recapitulate the various fantastical ideas that, from the highest antiquity, have been formed of this cause of the phenomenon of tides. Let it be sufficient in us to remark the immense distance it was necessary to pass from the system of hylozoisme, which contemplated the earth as an animal, whose alternate respirations produced the flux and reflux of the ocean, unto the physics of Kepler, who, at length, assigned positively the true cause of this phenomenon, by attributing it expressly to the universal attraction of matter.

We will limit ourselves here, in this hasty exposition, to direct the attention to the only posi-

tive theories, which have been produced on the phenomenon of tides, since their true cause has been perceived by Kepler.

Newton was thus the first, who, after his discovery of the law which governs this universal attraction, endeavoured to produce a positive theory of tides. But properly, it was on the occasion of the famous prize, proposed by the Academy of Paris, for the year 1740, that these researches became as general as they are important. D. Bernoulli, Euler, and Mac Laurin started with equal merit, shared the price, and smoothed, in some sort, this difficult career.

Bernoulli introduced into it the elliptic aqueous spheroid; Mac Laurin gave anew, for this spheroid, a deduction in all forms; and Euler, at length, displayed the value of the oscillations of fluids in the theory of tides. After this last geometer, D'Alembert, and especially La Place, in the Memoirs of the Academy of Paris, for the years 1775 and 1776, reduced almost entirely to this theory of oscillations, the theory of tides.

Unhappily, the results obtained by these different geometers, afford no agreement. Newton, Bernoulli, and Mac Laurin, found a result which would give, by the single action of the sun, a total tide of about $22\frac{1}{2}$ inches. Euler and D'Alembert found only 9. Simpson, who also occupied himself, with distinction, in this problem, obtained a like solar result, which would give 15 inches.

Clairaut followed very near Mac Laurin. And La Place only reconciled his results to experience by multiplying hypotheses, upon the depth of the sea, the resistance acted on the waters, and others similar.

Besides these theoretic essays, numerous observations, to ascertain all the circumstances of the phenomenon of tides, were made during the first half of the last century. To the Academy of Paris we owe those positive results: this learned body obtained from the government orders that there should be made, in the ports of France, a long train of observations; and, among the objects with which Richer was charged, in his voyage to Cayenne, the observations upon tides were recommended in an especial manner.* Recently, Captain Anderson of the British Marine, has made a course of interesting observations upon the tides and their progress between Fairleigh and the North-Foreland on the coast of England, or Cape Alpré and Calais on the French shore; and it is much to be desired, that this example was imitated in all other seas.

At length, after all these labours, the theory of tides was systematically re-produced by La Place,

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* We owe to the indefatigable La Lande, a precious collection of these various observations, which he published in 1781, in his *Treatise upon the flux and reflux of the Sea.*

in the fourth Book of his Celestial Mechanics. And therein is thus deposited the actual state of human knowledge upon the phenomenon of the flux and reflux of the sea. We ought, therefore, to fix here our ideas upon this knowledge

At first, for what concerns the means, instead of embracing the equilibrium of the entire surface of seas, La Place, as we have already said, confines himself to the consideration of the alternate motions or oscillations of the sea, in a point given on the surface of our globe.—He produces thus three species of oscillations, and cannot determine their respective extent, but in the hypothesis of an equal depth of the sea, and in that of a fluid regularly spread on the surface of an ellipsoid in revolution. Even in this case, purely hypothetical, and very limited, this geometer cannot obtain but the grossest of these determinations, dependent on the mass of the agitating stars divided by the cubes of their respective distances to the earth. La Place acknowledges himself, at N°. 16 of his theory, that the ulterior determination, which depends on the divided mass by the fourth power of the distances, is beyond the reach of the existing science; and he consoles himself by this consideration, that the observations of tides, made at Brest, do not appear to show a sensible result, depending upon this delicate term of the theory of tides.

Thus, considered in relation to means, this theory of the flux and reflux of the sea, appears to us, besides its imperfection, to be no other thing but a simple hypothesis.

Afterwards, for what concerns the results, La Place, guided anew by other learned conjectures, obtains, at No. 20 of his theory, an expression of the height of the tides; and at No. 21, he deduces from it the expression of the hour of high water.—It is this last result, reproduced expressly at No. 42, at the end of this theory, in order to serve for the calculation of tides, by means of a table, the construction of which this geometer proposes; it is, we say, this last result, serving to fix the time of high water, which is the only positive result in this last theory of tides, forming the actual state of our knowledge in respect to this phenomenon. Also, it is this result which has been adopted by the Board of Longitude of France, to serve, in the *Connoissances des Temps*, and in the *Annuaire* of this Board, for the annual calculation of tides.—Now, it is unhappily found, by the new theory which we submit to the British Board of Longitude, that this result of La Place, and consequently these calculations by the French Board of Longitude, are incorrect, in the open seas, at every place of the earth beyond the equator, and only agree with the truth at the single mo-

ment of the equinoxes, when the declination of the moon is zero.*

Thus, considered in relation to its results, this modern theory, which is the depository of human knowledge upon the phenomenon of the flux and reflux of the sea, is, in the single positive result which it has produced, erroneous, or at least inexact.—But, generally, embracing all the various theories of tides, what is decisive in their imperfection, is, that neither of them, without exception, can fix *a priori*, and without hypotheses, the absolute height of the tides, at least in the open seas. In that is the unexceptionable proof, that, in certain respects, as is the vulgar belief, the problem of the flux and reflux of the sea, remains still unresolved.

We think that these are motives sufficient for determining us to present to the British Board of Longitude, the new theory of tides, which results from the absolute theory of the construction of heavenly bodies, that we submit to this Board. We have already announced the results; we will only add here, that all those results are obtained

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* This assertion is proved rigorously in the subjoined work, wherein is to be found the complete expression of the hour of high water, for all geographic latitudes, and declinations of the sun and moon.

under the consideration of the influence of winds, whatever be their direction and strength. — As to the means we have employed in this new theory, it will be enough in us to say here that the absolute principle we have followed, is the continual and periodical displacement of a permanent equilibrium in the surface of the seas, whose only modifications constitute the divers local anomalies of the tides. This principle of a virtual permanent equilibrium is developed independently of all hypotheses, and rigorously, even to the delicate term which implicates the fourth power of the distances, what by La Place was conceived to be beyond the means of science.— The work is under the eyes of the Board, and they can thus see themselves whether these announcements are under or beyond the truth.

But, as much to facilitate the general inspection of those results, as to establish authentically their discovery, we ought, in this Address, to produce the form of the *Tide-Tables* that we present to the Board of Longitude, in advising that the numbers which are inscribed in these Tables, are those which, in the second volume of the manuscript work submitted to the Board, mark the formulæ that we offer, entirely finished, for the calculation of these Tables.—They are as follows :—

TABLE I. *Fundamental Circumstances of Tides.*

Epocha: 1821 January.		Culminating Point of the Aqueous Spheroid.			Polar Depression.	Formation of Tides.		
		Position		Height		Force	Resistance	Time
Days	Hours	Longitude	Latitude					
1	Noon	(142)	(143)	(147)	(144)	(148)	(185)	(151)
1	Midnight	..	..	..	..	..	..	..
2	Noon	..	..	..	..	..	..	..
2	Midnight	..	..	..	..	..	..	..
&c.	&c.							

TABLE II. *Local Circumstances of Tides.*

Ports, Harbours, Maritime Coasts, &c. &c.	Maritime Position.									
	Geographic Position.		Height of Tides.							
			Delay of Tides.		Oscillation of Waters.			Regulator of the Level.		
	Longitude.	Latitude.	Propagation of Waters.	Variation.	Annual.	Semi Diurnal.	Diurnal.	Solar.	Lunar.	
Plymouth, ...	..	..	(161)	(164)	(168)	(166)	(167)	(171)	(175)	
Brest, ...	..	..	..	..	..	..	..	..	..	
Cape Horn, ...	..	..	..	..	..	..	..	..	..	
&c. &c.										

With these tables, navigators, following very simple processes, that we explain, will be able henceforth, with facility, to calculate the state of tides for any time, and for every maritime place, whatever may be its position. The Board of Longitude may then, from this day, render annually to the British Marine this immense service, which, in certain respects, is, at the present moment, more important for its safety, than even the determination of the longitude, because, for the latter, they have already some approximations, whilst, for the determination of tides, all remains in a state of utter ignorance.

But, as it is seen in the second of these Tables, the various coasts and harbours exact, besides their geographic determination, another, constituting their *maritime position*. Happily, to fix this maritime position for every coast and harbour, it is sufficient, speaking rigorously, to make there only the observation of two consecutive high waters and the intermediate low water, and this at any time whatever, except the single instant of the equinoxes, when the moon is equally in the equator. Thus, navigators, approaching unknown coasts, will be able, in twelve hours, by means of these three single observations, to obtain data, which, for the future, will serve to fix, at any time, the state of tides in those waters.

At the time of the completion of these our labours, we have learned that the Board of Ordinance have caused to be continued, under the direction of Dr. M'Culloch and Captain Colbey, the geodesic measures performed by the lamented and erudite General Mudge.—The unceasing interest of this grand Institution for the trigonometrical survey of the British empire, induces us, with the utmost feelings of respect, to detach from the present work the practical application of those results which specially concern the determination of limited surfaces of our globe, that we might submit them, in time, to this Honourable Board. In fact, all that which, *as the base of Astronomy*, respects the general form of the earth, that is to say, the geographic determination of the whole globe, incontestibly belongs to the Board of Longitude, because the knowledge of the formation of heavenly globes, is an indispensable condition of celestial mechanics, on which astronomy is founded. But all that which, *as the base of Statistics*, concerns the particular form of the surfaces of political states, that is to say, the topographic determination of these limited surfaces, ought to belong separately to special political institutions in the various states; such as are the Board of Ordinance in England, the Board of Cadastre in France, &c.

On this occasion, where we are speaking of geodesic operations ordered by the Board of Ordinance, whose actual object, as we are told, is to rectify the results, in consequence of the deviation of the plumb-line, we think it our duty to make here two important observations.

The first is, that, according to the absolute theory we have produced, these corrections, dependent on the deviations of the plumb-line, are not only useless for the determination of the form of the earth, but further, are extremely fallacious. In fact, the true form of the surface of our globe consists in the *level of the water*, such as it establishes itself, with all those irregularities dependent on attractions varied by the divers masses situate at the surface of the earth. And it would be too arbitrary to be desirous of altering the natural level of water, for to discover the figure of the earth. Neither for geodesy, nor even astronomy, has the plumb-line need of rectification*. Such as it is in reality, this line proves every where the true form of the earth, which is not, and cannot

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* The only rectification that would be necessary to apply, if it were sensible, would be that of the continual variation of the level corresponding to that of the tides, the quantity of which our theory makes known, in fixing the *displacement of the zenith*, in consequence of the displacement of the waters by the tides

be any other thing but the continuity of perpendicular planes to the true line of the plumb.

The second observation is, that, in consequence of these great irregularities in the formation of the earth, which the new theory embraces generally, the geographic determination of a place on the surface of our globe, cannot be completed by the knowledge of its latitude and longitude. In fact, from this state of irregularity in our globe, it may possibly happen, that, of two places, the one which is nearest the equator and first meridian, might, rigorously speaking, have a latitude and longitude greater than the other place.

But these results are so very extraordinary, that we scarcely should here dare to speak of them by anticipation, if we could not remind the illustrious members of the British Board of Longitude, to whom we have the honour of addressing ourselves, that our theory we present them to legitimate all these announcements, is ABSOLUTE. We prove it at the end of the present work, in the last corollary; and, to solicit here instant pardon for that species of confidence with which this order of truths seems to inspire us, we beseech the Learned Board to observe, that this is the first ABSOLUTE THEORY that man has obtained in the grand domain of the Physical Sciences.

FINIS.